Archives of Medicine

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DEVOTED TO ORIGINAL COMMUNICATIONS ON MEDICINE, SURGERY, AND THEIR SPECIAL BRANCHES

EDITED BY

E. C. SEGUIN, M.D.

S'il est possible de perfectionner l'espèce
humaine, c'est dans la médecine qu'il faut
en chercher les moyens.
—Descartes

ELEVENTH VOLUME.

EDITORIAL ANNOUNCEMENT.

The announcement of the discontinuance of the AR-
CHIVES OF MEDICINE, made in its last issue, brought the
editor many protests from its former contributors and
friends. This, together with the fact that the editor has
given up his plan of returning abroad for a prolonged resi-
dence, led him to decide to continue the publication of the
journal.

As in the past, the ARCHIVES OF MEDICINE will appear
every two months, as punctually as possible, and will com-
prise about one hundred pages, more or less, according to
the material on hand.

Its distinguishing feature will be that only original ar-
ticles and reviews will appear in its pages. The editor be-
lieves that there is room in America for such a journal, in
addition to the monthly and weekly publications which
give abstracts, proceedings of medical societies, and medi-
cal news in such an admirable manner.

In the selection of original articles the editor will endeav-
or to provide for the representation of every department
of medicine; no specialty will be given any prominence
beyond that which naturally proceeds from the relative
activity of workers in the various branches of medicine.
Strictly scientific articles on anatomy, physiology, applied
chemistry and physics, and even psychology, will, in due
proportion to practical subjects, be welcome to its columns.
It is the editor's intention to incorporate related cases or
observations with the more formal papers, in the department
for original articles. This is justified by the intrinsic value
of well-recorded cases, and by the example of leading

European periodicals, in which important articles not seldom consist of one or two valuable observations with remarks.

The Editorial Department will continue to be devoted to topics of general interest, either by editorial remarks or by analytical and critical articles.

In the Review Department it will be the editor's aim to secure impartial and fearless reviews. During the five years' existence of the journal, only two or three reviews have called forth remonstrances from the authors of the books criticised; and in a recent instance the reply (?) to a perfectly just criticism consisted only of personal abuse of the reviewer—a proceeding which only confirms the reviewer's position.

As heretofore, illustrations will be freely inserted when necessary to the better understanding of the text and for the more precise record of cases.

During the past year Dr. Royal W. Amidon successfully conducted the ARCHIVES under peculiarly difficult circumstances, and the editor desires hereby to thank him for the faithful and very able manner in which he performed the task.

In conclusion, the editor, in soliciting the further support of the medical profession for this enterprise, will promise that on his part and on that of the publishers, no pains will be spared to make the ARCHIVES OF MEDICINE a first-class medical journal within its peculiar sphere.

Archives of Medicine.

Original Articles.

A PSEUDO NEGATIVE TRACE.

By S. E. POST, M.D.

PHYSICIAN TO THE OUT-DEPARTMENT OF THE NEW YORK INFIRMARY FOR WOMEN AND
CHILDREN.

1. There exists a pseudo negative trace.

2. This trace is the inverted image of a positive trace. It is produced by direction of impact against the frame instead of against the diaphragm of the explorer, and with the employment of about one half of the pressure which is in the latter case used.

3. The negative trace of the arterial pulse resembles the trace of the negative pulse of the veins. Both fall while the artery rises and rise while the artery falls. Both fall abruptly and rise in a series of curves. Their difference has not yet been made out.

4. The negative trace of the arterial respiratory curve resembles the trace of the respiratory curve of the veins. Both fall during inspiration and rise during expiration. The arterial differs from the venous trace, however, in the fact that the first reaches its minimum after the commencement of expiration, while the second reaches its minimum previous to the commencement of that act.

The existence of the above phenomenon first appeared to me during an observation of the superficialis volæ in a normal case. This artery lay directly beneath the skin, and its pulsation was apparent to the eye. Upon either side and separated from it by the half of an inch were its accompanying veins; they also could be distinctly seen. The Pond sphygmograph was used with the production of the negative trace, which is shown in fig. 1. This trace was produced when the explorer was so placed that one side of its

frame only rested upon the artery, its diaphragm being supported by the adjacent part.

Trace I fell while the artery was rising, and it rose while the artery fell; also an uplifting of the entire instrument could be observed with each impulse from the arterial wall. When the explorer was moved to a position over the artery the usual trace, opposite in character, appeared; and when it was removed from it, toward either side, the needle hung motionless. Trace I required arterial impact, but impact only against the frame. In explanation, it is now proposed that the diaphragm, pressed upward by its apposition to the part, would lose its support with the elevation of the frame, and that consequently, with each impulse, the
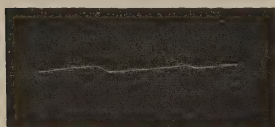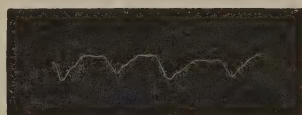

FIG. 1.

FIG. 2.
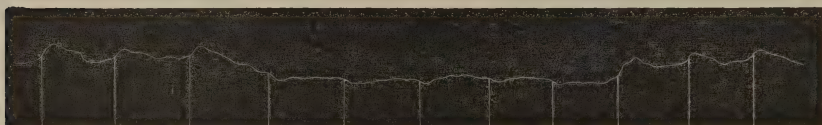
FIG. 3.

diaphragm fell. The above principle, once true, is always true. Based upon it, the trace produced by impact against the frame must be opposite in character to that produced by the same impact when applied to the diaphragm, while the accomplishment possible to the feeble superficialis volæ may be repeated by any of the larger available arterial trunks.

These propositions derive importance from their relation to propositions concerning the normal venous pulse or the negative pulse of the veins. As is now known, this pulse consists of an abrupt fall of venous pressure, which is synchronous with the contractions of the ventricles, and which is due to a periodic acceleration of the flow of blood into the

heart. Though its existence is said to have been advocated in 1752 by Lamure,[1] rejected in 1759 by Haller,[1] and again advocated by Carson[1] in 1824, by Wedermeyer[2] in 1828, by Weyrich[2] in 1845, by Berthold[1] in 1869, and by Cappie[1] in 1874, its modern interests must be considered to date from the publications of Mosso[3] in 1878 and 1879. This observer first obtained from the jugular vein a tracing which distinctly fell during the elevation of the carotid wall. Following Mosso, Franz Riegel,[4] May 2, 1881, stated that he had observed in dogs a venous pulse which alternated in time with that of the arteries, also that in many healthy men he had obtained a pulse which did not differ from that he had obtained in dogs. He summed up by saying : " There exists a normal venous pulse. This pulse falls during the heart's (ventricle's) systole, and rises during its diastole. It is explained by the fact, that during the systole of the heart (ventricle) the flow of blood from the veins is accelerated, while during the diastole of the heart it is retarded." May 14, 1881, Gottwalt[5] presented a paper entitled " The Normal Venous Pulse." Gottwalt's observations were upon dogs. The vein was dissected from its surroundings, under it was placed a concave tambour, and over it a closely fitting metal plate. Gottwalt's curve consisted of one large wave preceded by two smaller ones. So nearly as he could tell, the second sound of the heart corresponded with the apex of the first smaller wave.

In April 1882, François Franck[6] gave traces taken simultaneously from the jugular vein and from the heart.

The tracing from the vein is similar in form to that given by Riegel. Following the sequence of the cardiogram,

[1] *Jour. de méd. de chir. et de pharm.*, Jan., 1883, p. 48.
[2] Hermann's " Hdb. der Phys.," Bd. IV., Th. I., S. 331.
[3] Hermann's " Hdb. der Phys.," Bd. IV., Th. I., S. 178.
[4] Riegel : *Berl. klin. Wochenschr.*, May 2, 1881.
[5] Gottwalt : *Pflüger's Arch.*, May 14, 1881.
[6] Franck : *Gaz. hebd.*, April 7, 1882, p. 225.

however, it is supposed to consist of : 1, a sudden rise and descent at the commencement of the total curve ; 2, a second rise and descent ; 3, a gradual ascent, and then again a sudden rise, indicating the commencement of a new series. The sudden rise and descent were synchronous with the contraction and relaxation of the auricle, and occurred with equal promptitude when the contractions of the ventricle were arrested by stimulation of the pneumogastric nerve. The second rise accompanied the ventricular systole.

His explanation is as follows : The sudden rise is due to increased pressure from obstructed flow at the instant of the contraction of the auricle, and the sudden fall to rapidity of the current at the commencement of the diastole, when the blood precipitates itself into the relaxing auricle. The second rise is due to retardation of the current from the loss of the aspiratory influence of the auricle, and the second fall to a new acceleration from relaxation of the ventricle, while the gradual ascent is due to retardation from the gradual repletion of the whole right heart.

Further communications upon the subject have been made by Riegel [1] since this time, but these are chiefly explanatory and do not add any new facts.

To recapitulate : the negative pulse of the veins consists of a diastolic ascent prolonged and broken into waves, and of a descent which is systolic and abrupt. In the words of Riegel, it is in form and in time opposed to the normal arterial pulse. The above description is also absolutely true for the arterial sphygmogram from frame impact as presented in this paper and as illustrated in fig. 4. It is conclusive, therefore, that the trace thus produced should be eliminated from all considerations relative to the venous curve. A short time ago the writer [2] presented tracings

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[1] Riegel: *Deutsches Arch. f. klin. Med.*, May 30, 1882; *Volkmann's Sammlung klin. Vorträge*, March 13, 1883.

[2] *N. Y. Med. Record*, Feb. 17, 1883.

supposed, from their systolic descent, to represent this venous curve. The tracings from the external jugular were from pulsation which was apparent to the eye. With the exception of these the pulsations supposed to have been registered, were not seen, and in each instance the instrument was in a position to have received arterial frame impact; while from a similarity in the number and position of the secondary waves to the number and position of the secondary waves in the inverted arterial trace a similar origin must be inferred. Also the circumstances found to accentuate the supposed venous trace were such as would accentuate a trace from the arterial pulse.

Many of Riegel's tracings also show this likeness between the venous curve and the inverted arterial curve of the part, notably figs. 5 ; 7, *a* and *b* ; 8, *a* and *b* ; and 9, *Deutsches Arch. f. klin. Med.*, May 30, 1882, and fig. 2, *Volkmann's Sammlung*, March 13, 1883. In the presence of this similarity we should have evidence that the possibility of frame impact had been avoided.[1]

Other interpretations also require to be reconsidered from the stand-point of this possibility. Dr. Ebenau[2] recently presented a negative trace as the trace of an arterio-venous aneurism, which was afterward dissected and tied. The instrument was applied over the aneurism, which involved the femoral vein. The Marey sphygmograph was used. His inference was based upon the argument that no other explanation remained for the trace, as the normal venous pulse did not extend so far toward the periphery, while there existed no lesion of the tricuspid valve. His instrument also was in a position to have received arterial frame impact.

More recently Dr. A. T. Keyt[3] has presented negative

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[1] Gottwalt's observations were upon isolated veins, while Franck's explorer was supported above the surface of the part.

[2] Ebenau : *Berl. klin. Wochensch.*, January 8, 1883.

[3] Keyt : *Boston M. & S. J.*, October 11, 1883.

tracings as representing "The Negative Arterial Pulse," supposed to be a pulse of the tissues adjacent to the artery. The illustrations which accompany this paper refute its theory. Its theory requires aspiration of the skin under the explorer, this aspiration to be dependent upon change in the position of the arterial wall. Its author finds the arterial pulse to consist of two movements, expansion of the lumen of the artery and locomotion of the whole vessel from its bed. Of these movements locomotion only is capable of effecting aspiration—expansion would tend to diminish it. Further, the perivascular tissues are not in a condition of vacuum. They are a mass of freely communicating lymph-spaces upon which the aspiration would primarily be spent. The trace of skin-aspiration by the arterial pulse, then, would equal in amplitude the trace of the arterial pulse minus the influence of expansion and minus the influence of the aspiration of lymph. Compared with the pulse-trace it would be a curve of a limited extent. The author's negative trace is, on the contrary, in some cases an inverted image of his positive trace, while the position of his instrument was such as would favor arterial impact against the frame.

Not only the sphygmogram but also the cardiogram may be negatively fac-similed by frame impact. An instance is shown in fig. 3. Fig. 3 was obtained from a normal heart, the explorer being applied in the fifth intercostal space. Its transition from positive to negative was effected by moving the loosely adherent skin upward until the lower border of the explorer rested in the centre of the space, and that from negative to positive was effected by its return. That the negative descent was systolic is shown by the spacing of the trace. Systolic sinking of the chestwall has been noted in the third and fourth intercostal spaces with hypertrophy of the heart.[1] The heart occupies

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[1] Landois : "Lehrbuch der Phys. des Mensch.," Th. I., S, 98. 1883.

less space during systole than during diastole.[1] The cardiac systole has an aspiratory force, but that force is principally expended upon the lungs, whose volume increases during systole and decreases during diastole with the rhythm of a pulse.[2] The influence of heart-aspiration upon the thorax, therefore, must be slight. This heart also was normal and feeble in its systole rather than otherwise. Further, the diaphragm of the instrument rested upon the fifth rib. The production of the trace by aspiration, hence, was impossible.

Without moving the explorer, the cardiogram does at times pass from positive to negative under the eye. Franck[3] has published a tracing which was thus obtained. Franck's tracing was obtained during a profound inspiration. He considered it to imply predominating effort at systole from an exaggerated repletion of the heart. Franck's sphygmogram simultaneously taken shows increased height of the percussion stroke, but does not show any systolic irregularity of the heart. Repletion was not present in my case, and further, the negative trace was produced at will by simply altering the position of the instrument so that impact could occur against the frame. Upon the theory of this occurrence, spontaneous change to the negative also can be explained by advancement or recession of the apex from increase or diminution of systolic force.

Fig. 2 is also a cardiogram; it is presented in this connection only as a more perfectly formed trace. It also is to be read from the left to the right. Trace 2 was obtained from a case of mitral stenosis with dilatation of the heart. Cyanosis was marked, and a pulse was present in the external jugular vein. This pulse was distinctly negative.

[1] Marey : "Trav. du lab.," p. 51,1875. Franck : "Trav. du lab. de Marey," p. 187, 1877.

[2] Voit : *Ztschr. f. Biologie*, I., S. 390, 1865.

[3] Franck : " Trav. du lab. de Marey," p. 59, 1876.

It fell with the arterial impact. The descent of the cardio-
gram occurred at the same time. Though repeated at-
tempts were made, a positive cardiogram could not be ob-
tained in this case.

FIG. 4.

a. From the left radial by impact against the frame.
b. Simultaneously trom right radial by impact against the diaphragm of the
instrument.

The persistently negative cardiogram has been associated
with mediastinitis and the resulting adhesion of the peri-
cardium to the chest. Systolic sinking did not occur in this
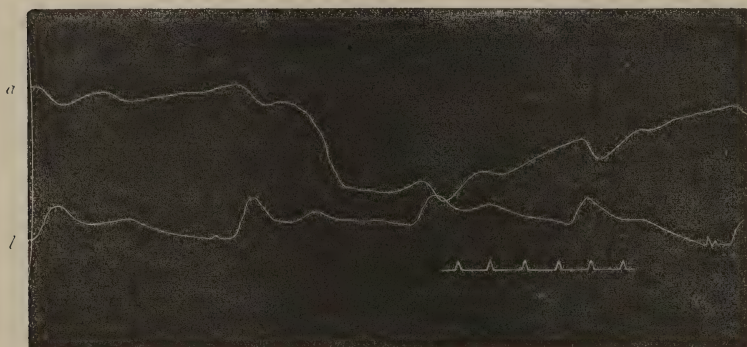

FIG. 5.

a. From left radial by impact against frame.
b. From right radial by impact against diaphragm.

case. On the contrary, a wave of expansion was visible,
which covered a considerable area. The explanation of
fig. 2 probably lies in this diffuseness of the beat, which
would be unable to lift separately the limited surface cov-

ered by the diaphragm, and which could produce any trace only when lifting one side of the frame.

The respiratory blood-pressure curve is also capable of falsification by this same method. Fig. 5, *a* and *b*, show simultaneous tracings from the two radial arteries: *b* produced
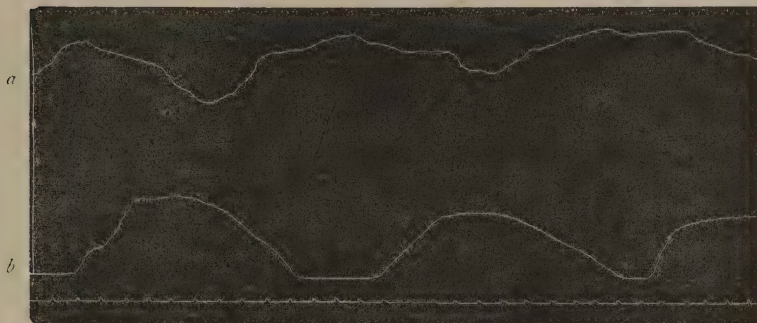
FIG. 6.

a. From the carotid by impact against the frame.
b. From the sternum by impact against the diaphragm.

by impact against the diaphragm, and *a* produced by impact against the frame. The single pulse-curve of *a* presents the same opposition to the pulse-curve of *b*, which is shown in fig. 4. The two tracings of fig. 5 present, in addi-
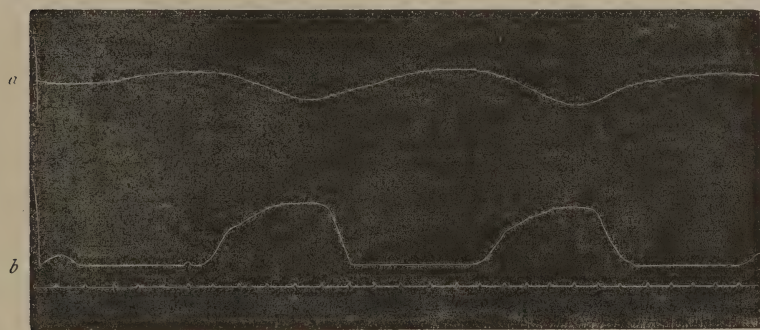

FIG. 7.

a. From the ext. jugular vein by impact against diaphragm.
b. From the sternum by impact against diaphragm.

tion, lines of respiratory variation in which also *a* opposes *b*. Here, however, we do not find an inverted image, as in the case of the pulse-curve. The negative respiratory line

shown in *a* is apparently exaggerated. It is not really ex-
aggerated, the respiratory line in *b* is repressed, as respira-
tory variations do not require the pressure which is em-
ployed to register the pulse.

The negative respiratory line would be a matter of small
importance did it not also simulate a real phenomenon. In
normal respiration the arterial blood-pressure rises during
inspiration and sinks during expiration, while the venous
blood-pressure sinks during inspiration and rises during ex-
piration. The negative trace of the arterial variation will
also fall during inspiration and rise during expiration, and
hence approach the character of the venous curve. It is
not, however, its exact counterpart. The respiratory rise
in arterial blood-pressure reaches its maximum after the
commencement of expiration.[1] Its negative trace will reach
its minimum necessarily at the same period. This occur-
rence is shown in fig. 6, *a* and *b*, taken simultaneous-
ly,—*a* from the carotid artery by frame impact, and
b from the surface of the thorax by impact against the
diaphragm.

The respiratory variation of blood-pressure in the vein, on
the other hand, though also falling during inspiration and
rising during expiration, possesses a distinguishing mark.
It reaches its minimum previous to the commencement of
the expiratory act. This peculiarity has been recent-
ly considered by Franck.[2] It is rudely represented in
trace 7, *a* and *b*, trace *a* being obtained with the ex-
plorer over the external jugular, and trace *b* simul-
taneously by an explorer applied to the thoracic wall.
Impact occurred against the diaphragm in both instances.

To conclude, negative traces from impact against the
frame are capable of producing confusion in otherwise

[1] Foster's " Physiology," p. 344, 1879.
[2] Franck: *Gaz. hebd. de méd. et de chir.*, March 10, 1882, p. 157.

valuable evidence.   A possibility of frame impact should
be considered in all deductions relative to a negative
trace.

The compound sphygmograph used in these observations
was the Keyt, kindly loaned to me by Dr. M. P. Jacobi.
Similar negative sphygmograms have been obtained also
from the Dudgeon instrument whose diaphragm and pedestal
are upon the fashion of the Marey, while its conducting and
registering apparatus resembles that of the Pond.

ON ANGIOMA OF THE LARYNX.

By LOUIS ELSBERG, M.D.

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ANGIOMATA, *i.e.*, true blood-vessel growths, are among the rarest of all intra-laryngeal tumors. Aside from the two cases I am about to relate, I know of but three recorded instances.

CASE 1.—On July 10, 1876, Mr. E. H., a merchant, thirty-seven years old, consulted me on account of an increasing hoarseness which had troubled him from time to time for over six years. Born of healthy parents, and never having been seriously ill in his life, he had contracted during a twelve years' residence in Brooklyn a naso-pharyngeal catarrh which he thought had gradually involved the larynx. During the last two years it has somewhat affected his general health and strength, though with the exception of dyspeptic symptoms all his functions are well performed. Since last spring he lives in New York City, and thinks his catarrh is a little less troublesome ; nevertheless every morning, on rising, the phlegm nearly chokes him, and he must hawk and spit a great deal the whole day. For half an hour after he has thoroughly cleaned out his nose and throat, he finds that his voice is worse than at any other time, but in the course of an hour or two becomes better, and remains nearly normal for several hours ; then it " hoarses up," and becomes more and more husky, until about half an hour after the next clearing out. He coughs very little, and has no pain in any part of his throat or respiratory tract.

The patient is a very energetic man engaged in a business requiring him to use his voice a great deal. While relating the above history of his case (at about 10 o'clock in the morning) his

voice sounded but little veiled. Examination of his chest ex-cluded both pulmonary and cardiac trouble, either the one or the other of which the intelligent but rather nervous patient had feared. I found catarrhal congestion and hypertrophy in his nose and throat, blood-vessels abnormally visible on the epiglottis and on the vocal bands, and attached to his right vocal band a round black tumor the size of a pea. There appeared to be a thin dark pedicle adhering lengthwise to the vocal band, extending from the anterior commissure for fully an eighth of an inch along the edge, and the tumor, round though it was, had a portion of its periphery likewise attached to the vocal band so that nearly half an inch of what should have been a free vocal edge was involved. Looking at the laryngoscopical image, as sketched in fig. 1, the clearness of the patient's voice was astonishing to me.
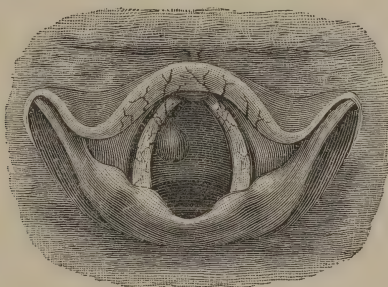
FIG. 1.

Mr. H. bore the application of the mirror remarkably well, and after a little drilling and a few fruitless attempts, I succeeded during his first visit in snaring off the tumor with Gibbs' wire loop. Considerable hemorrhage ensued immediately, which was pro-longed by violent coughing, straining, and peculiar expiratory efforts of the now intractable patient.

After twenty minutes or half an hour, the patient allowed me to inject a nebulized solution of persulphate of iron, which in less than five minutes arrested the bleeding. Laryngoscopical examina-tion after this was unsatisfactory. I prescribed inhalation of the nebulized solution for a few minutes every two hours.

The tumor which had adhered to the wire loop when re-moved was bluish-black and the size of a pea, but it soon lost its dark color and became grayish. Later, on micro-scopical examination, it showed a fine spongy structure

composed of trabeculæ of connective tissue containing blood and blood-vessels, so that the diagnosis of the tumor as *angioma cavernosum* was indubitable.

On the next day the larynx was very painful and there was aphonia, but no more bleeding had taken place. The pedicle, adherent to the right vocal band, and a stump of the tumor were distinctly seen. The inhalation was ordered to be continued every three hours.

July 12th. Patient has slept badly; is feverish; tongue furred, bowels constipated. Aphonia persists, but laryngeal pain has diminished. The stump of the tumor appears shrunken. Prescribed a purge of calomel and jalap; inhalation only three times daily.

From this time on, the patient's voice improved rapidly. Local applications of my solution of iodoform in ether (3 ij ad 3 j) were made in the interior of the larynx, and to the mucous membrane of the pharynx and nose. The inhalation was discontinued after about a week.

Two months later no trace of the tumor was visible. Even the pedicle had disappeared. The patient's voice was perfectly satisfactory, and his catarrh had ceased to be troublesome.

No recurrence has taken place.

CASE 2.—J. C. C., æt. twenty-eight, of healthy parentage, presented himself to the New York Polyclinic, on Oct. 15, 1883. As an infant he has had whooping-cough, later diphtheria, and for many years catarrh. His voice was very clear until about five years ago. At that time he was lecturing with Edison's Phonograph ("Palingenophone"), and once gave eight different lectures of one hour each, in one day. On the next morning he could scarcely speak above a whisper. This was the first time in his life that he was hoarse, and, though his voice soon became comparatively clear again, he never got entirely free from an occasional, more or less, slight huskiness. Four years ago he came to New York to study vocal music, and for one year his voice improved very much; but three years ago last June he began to notice an increasing falling off of power and purity of tone; at times he

could not sing at all. He then consulted Dr. W. W., who thought his tonsils, which were enlarged, caused the trouble, and removed them. For a time it seemed he was getting better, but by degrees he became worse again. One year ago last August it was only with great effort that he could speak plainly ; under treatment his voice improved, so that he could sing over an octave, but the relief was only temporary. In January, 1883, his vocal instructor, Mr. K., on examining the larynx, discovered a tumor on his right vocal band, the existence of which was afterward confirmed by his physician. At that time he could produce, with an effort on his part, one octave and a half of almost clear tones, and another physician, who examined him with the laryngoscope, said there was no tumor, but that he was suffering from " a partial paralysis of the vocal cords." He was treated with the electric battery, and was assured that the hot months of the coming summer would cure him entirely. But instead of getting better his voice became worse. Since August, he has gone to the Manhattan Eye and Ear Infirmary, the New York Eye and Ear Infirmary, and the clinique of the College of Physicians and Surgeons, and has been examined by a large number of doctors and students.

On admission, his general health was found to be excellent. He is married, and has never had any venereal disease. His conversational voice is distinctly husky, but he can produce single notes of considerable clearness and musical clang, ranging through
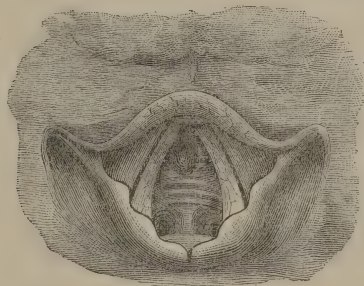
FIG. 2.

nearly an octave of a baritone voice. Aside from this condition of his voice, he has, he says, absolutely nothing to complain of. Examination revealed, in addition to catarrhal condition of the nose and throat, a reddish-black, smooth pyriform tumor, attached to the edge of the right vocal band, extending from the anterior commissure back for at least a third of its length, as shown in fig. 2.

Although the laryngoscopic examination of this patient was ex-

ceedingly easy, and his well-proportioned fauces and pharynx tolerated the use of my largest-sized mirror (having a diameter of 3½ cm.), it was far from easy to ascertain that the tumor was temporarily indentable though of rather firm consistence. The introduction of instruments into the larynx was very badly borne; nevertheless, the diagnosis of angioma was made from the appearance of the tumor at the first visit to the Polyclinic.

The catarrhal condition was ameliorated by means of cleansing and alterative applications of my ethereal iodoform solution, and I had to go through a regular system of training for the purpose of gaining access to the tumor. Its precise attachment, and the circumstances of the case, made its proper removal—especially in a patient who depended for his future livelihood upon his singing voice—one of the most delicate and difficult of operations.

On Nov. 23d, I attempted its evulsion with Cusco's forceps, of Robert & Collin's make, but on account of the peculiar bend of that instrument with its antero-posterior opening, I succeeded in pinching off only a small portion of the posterior and lateral end of the tumor. Some bleeding was the result at the time, and some sloughing and diminution of the size of the tumor followed after. On Nov. 30th, I used my modification of the Cusco forceps,—*i. e.,* one with a bend of one and a quarter right angle,—seized the whole of the tumor close to the edge of the vocal band and its anterior insertion, compressed the tumor forcibly, and squeezed it off— bringing it out in the hollow of the forceps. Less bleeding took place than when I pinched off the small portion, and a slight cauterization with nitrate of silver completely arrested it very soon. I insisted upon the patient's absolute vocal rest, and for several weeks he wrote all he wished to communicate. I also attended to his general health and regimen, and kept him from all physical and emotional excitement. For some days there was a good deal of suggillation seen in the right vocal band, and hyperæmia and congestion of the part remained for more than three weeks. But he has recovered completely, and no vestige of the tumor can be detected on the closest examination.

At the meeting of the New York Pathological Society, December 12, 1883, I presented this tumor,[1] microscopical examination of which confirmed the diagnosis of angioma.

[1] See Transactions of New York Path. Soc., *New York Medical Journal,* January 12, 1884, p. 51 ; *N. Y. Med. Record,* January 5, 1884, p. 21 ; *Philadelphia Medical Times,* December 29, 1883, p. 264 ; etc.

A. Definition and Synonyms.

Angioma is a tumor consisting of a blood-vessel growth. It is not simply a blood tumor or hæmatoma, and differs from a varix or an aneurism, *i. e.*, a dilatation of either venous or arterial blood-vessels on the one hand, and a vascular cyst on the other. According to Virchow [1] no growth ought to be regarded as an angioma which does not essentially consist of newly formed blood-vessels, or of blood-vessels with newly formed elements in their wall. Heitzmann says : " The characteristic feature of angioma is an abundant supply of blood-vessels—arterial, venous, or capillary. This causes its erectibility, *i. e.*, its swelling on a spontaneous engorgement of the vessels." [2]

The name " Angioma " is preferable to " Angionoma," which latter is the term used by J. Hughes Bennett,[3] and Follin,[4] but which, as Virchow has pointed out, is etymologically incorrect.

As synonyms the terms " vascular tumor " and " erectile tumor " have been employed, and by different authors also a number of others which have given rise to much confusion.[5]

B. Cases Observed.

I have stated that in addition to my own two cases I know of but three instances of intra-laryngeal angioma. These are the following :

CASE 3.[6]—Dr. Fauvel, of Paris, was consulted on June 4, 1872, by a merchant of Lille, fifty-three years old, in good general health, who, without known cause had become

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[1] Die krankhaften Geschwülste. Von Rudolf Virchow. Berlin, August Hirschwald, 1867, vol. III., p. 308.

[2] Microscopical Morphology of the Animal Body in Health and Disease. By C. Heitzmann, M.D. New York, J. H. Vail & Co., 1883, p. 512.

[3] Clinical Lectures on the Principles and Practice of Medicine. Edinburgh, 1858, p. 156.

[4] Traité élémentaire de pathologie externe. Paris, 1861, vol. I., p. 204.

[5] Compare Virchow, *op. cit.*, p. 307, *et. seq.*

[6] Traité pratique des maladies du larynx. Par le Dr. Ch. Fauvel. Paris, V. Adrien Delahaye et Co., 1876, p. 545. (Observation 185.)

hoarse about a year previously. There was no cough, no feeling of foreign body in the larynx, no pain. He breathed without difficulty; auscultation elicited nothing wrong in his chest; there was no specific history. The laryngoscope showed in the angle of the vocal bands, on the level of their anterior commissure, attached on the right side, a non-pediculated red tumor, blackish in places, about as large as a filbert. After prescribing bromide of potassium for six days, and accustoming the patient to the contact of his forceps, Fauvel removed the tumor by evulsion in five separate sittings. Some bleeding ensued each time, and for some days the bands appeared red and ecchymosed. Microscopical examination proved the tumor to be angiomatous. The patient was cured and no recurrence took place.

CASE 4.[1]—On May 1, 1873, a man, thirty-seven years old, came to Dr. Fauvel on account of a severe hoarseness which had lasted about seven months. At times his voice was clear, but suddenly, without appreciable cause, it gave way. Laryngoscopical examination revealed on the free edge of the left vocal band, very near its anterior insertion, a tumor of the size of a pea, blackish red, smooth, slightly pediculated, which prevented the approximation of the vocal bands. The patient had no idea of the presence of the tumor; he had no sensation of a foreign body; he did not cough; breathed without difficulty, and, as he said, if he were not hoarse, would be in perfect health. After the patient had taken some bromide of potassium, ineffectual attempts of removal of the growth were made on May 5th, 8th, 12th, and 15th. On May 19th Dr. Poyet, Fauvel's chief of clinic, pinched off the little tumor with forceps without teeth. The patient felt nothing, and the voice at once became clear and of good quality. In the larynx was seen only a little bleeding point where a moment before the tumor had been.

[1] *Ibid.*, p. 606. (Observation 240).

The cure was complete. No recurrence occurred. Microscopical examination proved the tumor to be a cavernous angioma.

CASE 5.[1]—In 1878, a railroad official, thirty-eight years old, looking somewhat anæmic and weak, who, with healthy lungs, had long suffered from chronic pharyngeal and laryngeal catarrh and veiled voice, but had no other symptoms and no idea of a tumor in his larynx, had come to consult Dr. Heinze, of Leipzig, merely on account of his catarrh. Aside from his catarrhal condition, Dr. Heinze found at the border of the right ventricular fold, looking out from under it, near the anterior commissure, a tumor of the size of half a lentil, and behind it, separated by a free interspace, another tumor about half this size, both of the color of a black berry, roundish, and uneven. The entrance to the right ventricle appeared about twice as broad as usual and as that of the left side ; and the fold, forming its upper boundary, was pushed up like a dome, especially in its upper portion. To the touch with the probe, both tumors appeared soft, with depressable surface. When the parts had been sufficiently educated to allow the ventricular fold to be pulled away a little with the end of the probe, it was found that the growth came from the depth of the ventricle, and that the two black projections seen at the border of the fold were portions of one and the same neoplasm. The anterior projecting portion—the larger— was removed with an ordinary wire loop ; an extremely violent hemorrhage, lasting nearly half an hour, ensued from the stump. About a cupful of bright-red blood was lost before the bleeding was arrested by cauterization with silver nitrate ; it recommenced in the evening, and was definitely stopped only by another and very energetic cauterization of the whole ventricular fossa. Very soon after, the smaller projection had become invisible, obviously be-

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cause the main tumor had shrunken ; but in the course of three months, a little black elevation could be detected in the same spot where the larger tumor had been. This enlarged slowly but steadily, and in ten months had attained to the size of a pea. Dr. Heinze destroyed the projecting portion of this neoplasm by repeated punctures with a sharp-pointed galvano-caustic instrument, and cauterized the entire fossa with a flat galvano-cautery bent on its side. These operations produced almost no pain, and but insignificant bleeding. No recurrence has taken place.

To these five cases I might add a sixth—viz., that of an angio-papilloma, which occurred in the practice of Dr. Fauvel, and which that author reports as a " Papillary polyp, with very pronounced vascular dilatations, making it resemble an angioma." [1] A man, twenty-eight years old, consulted Dr. Fauvel, April 1, 1871, having suffered for two years with a progressive hoarseness that made him fear complete loss of voice. His health was perfectly good, but there was seen, at about the middle of the free edge of the right vocal band, a tumor of the size of a pea, with broad attachment, rounded and smooth, which interfered with the approximation of the vocal bands during phonation. Its color was blackish. The patient took bromide of potassium, in large doses, for two months, and on June 6th, Dr. Fauvel succeeded in removing the tumor by evulsion, but the bleeding was more than usually abundant. The part was cauterized with silver nitrate. The vocal bands, at first very red, recovered in a few days, the patient's voice became normal, and no recurrence of the tumor took place.

Dr. Heinze, in his article in the *Archives of Laryngology*, mentions three cases observed respectively by Drs. Johnson, Fournié, and Mackenzie, which, however, are not cases of intra-laryngeal angioma.

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[1] *Op. cit.*, p. 522.   (Observation 163.)

The case of Johnson [1] referred to, a man fifty years old, who consulted the Dr. first in October, 1863, on account of hoarseness, which was much increased by continuous speaking even for a few minutes. Feebleness of the voice had then existed for more than a year. When Dr. Johnson first examined the patient, he saw only the injected state of the mucous membrane, but after repeated examinations he found a red and apparently very vascular tumor between the two vocal bands·just at the anterior commissure. Repeated applications of solution of iodine by means of a bent brush were made without decided or permanent benefit, and on June 27, 1865, Dr.·Johnson attempted to catch the tumor with a wire écraseur. There was an escape of about an ounce of blood, and in the midst of it a shreddy piece of membrane was found. The laryngoscope showed that the tumor was gone ; in its place there was a slightly uneven projection near the anterior extremity of the right vocal band where the tumor had been attached. This spot was occasionally touched with a solution of silver nitrate, and the nodular unevenness quite disappeared. The voice gradually became stronger and clearer. No recurrence took place. Dr. Johnson inferred that the tumor was a vascular cyst containing fluid, that the cyst had been ruptured by the grasp of the wire loop, and that the membrane expectorated had formed a portion of the cyst-wall.

Dr. Fournié [2] has reported two cases of melanotic tumor : One, in a lady, about forty-six years old, suffering from obstinate and severe cough. The tumor was situated in the right vallecula, implanted with a pedicle at the level of the free border of the epiglottis, and overlying the right arytenoid cartilage, and was removed by means of a gal-

[1] "A History of Seven Cases of Growth on the Vocal Cords." By George Johnson, M.D. Transactions of the Medico-Chirurgical Society. London, vol. LI., Case 2, p. 176.

[2] "Tumeurs mélanotiques du pharynx" par Dr. Edouard Fournié. *Gazette des hôpitaux*, No. 56, 1868, p. 223.

vano-cautery platinum loop-knife, which was made to en-
circle it, and was drawn through the tumor with slight
traction.   The other was a tumor found in a man about
thirty years old, in the angle of the pharynx on the right
side extending into the pyriform sinus.

Mackenzie's case [1] was that of a captain of the Royal
Marines, aged thirty-five years, who was sent by Dr. Smyly
in December, 1869, complaining of an uneasy tickling sen-
sation in the throat, which had come on in the previous
summer, when he had hay-fever.   The voice was normal.
On laryngoscopical examination, a growth, the size, color,
and configuration of a ripe blackberry, was seen in the right
pyriform sinus.   Several attempts were made at removal,
but owing to the extreme hardness of the growth, it was
not entirely removed till January 21, 1870.   Incomplete
evulsion was effected with tube forceps, and the base of the
tumor was excised with cutting forceps.   The operation
was attended with more hemorrhage than is usual, and
slight after-bleeding took place for some hours.   After its
removal, the neoplasm lost its black color, and became pink-
ish-red.   On section its appearance to the naked eye was
compact, and its color almost white.   On microscopic ex-
amination, its structure was seen to be obscurely fibrous,
the fibres being very closely intermingled, and the vessels
so closely and tightly packed together that they could not
be isolated.

In addition to the two cases just mentioned, of Fournié
and Mackenzie, I know of two other instances of melanotic
or angiomatous tumor in the pyriform sinus, one ob-
served by Fauvel, and one that has occurred in my own
practice.   It is remarkable that in all these cases the tumor
was on the right side.   Fauvel's patient [2] consulted him

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[1] Essay on Growths in the Larynx.   By Morell Mackenzie, M.D.   London,
J. and A. Churchill, 1871, Case 89, pp. 188 and 53.

[2] *Op. cit.*, p. 882.

October 15, 1867. He was a man, thirty-five years old, who had for several years been suffering from an irritating, dry cough produced by a tickling sensation in the region of the upper border of the thyroid cartilage on the right side. Otherwise perfectly healthy, he had in his right pyriform sinus a mammillated black growth as large as a bean, which Dr. Fauvel diagnosticated as a melanotic or angiomatous tumor. He destroyed it by three applications, with an interval of five minutes between each, of Voltolini's galvano-cautery. The burning was very bearable and there was no bleeding. The tumor was reduced to a very small escharotic point on the outer posterior laryngeal wall, which remained for a few days and then disappeared. The patient was cured of his cough and morbid sensations, and no recurrence took place.

The subject of my case of angioma of the pyriform sinus was a married lady, fifty-two years old, anæmic and weak, and mother of several children, who consulted me in June, 1873. She had a most annoying, constant inclination to clear her throat, and insisted that she had something grow-
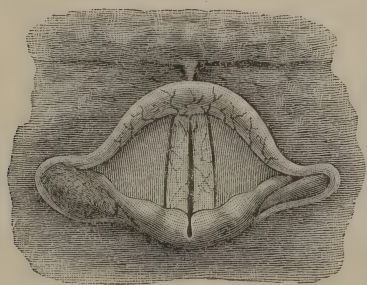

FIG. 3.

ing there. Her voice was unaffected, but she had a peculiar cough, and pain in swallowing. So many physicians had prescribed for her without giving her the least relief that she was greatly discouraged and melancholic. She pointed out the laryngo-pharyngeal region of the right side as the

seat of all her trouble. She had never been examined with the laryngoscope. The latter revealed a bluish-black rough-looking, flat tumor, as large as the end of my little finger, filling out and stretching the right pyriform sinus (see fig 3).

The tumor was easily touched, but bled on slight provocation. I applied to it daily for a while a diluted solution of persulphate of iron; afterward, every other day, tannin and glycerine; and still later a few times, my ethereal solution of iodoform. On the third of July I removed a portion of it with my ordinary laryngeal forceps. This operation was followed by considerable hemorrhage, which, however, was stopped in fifteen or twenty minutes by the injection of nebulized solution of persulphate of iron. The portion of tumor removed proved to be of angiomatous character, and I destroyed the remaining portion *in situ* by repeated applications of the galvano-cautery. These galvano-caustic operations were usually also followed by some bleeding, but not enough to require recourse to hæmostatic measures. On July 29th the patient left the city cured, and no recurrence had taken place when I saw her five years later.

C.—Pathology and Etiology.

The etiology and pathogenesis of angiomata are by no means clear. Although such growths in other parts of the body are frequently congenital, there is no evidence that any case of laryngeal angioma has existed from birth. On the contrary, on the one hand, the cases observed seem to have clearly had a beginning after adult age had been attained; and, on the other, not a single one of the congenital intra-laryngeal tumors hitherto observed has been angiomatous.

Laryngeal angioma appears to be confined to the mucous membrane and to be accompanied by, or perhaps rather to accompany, chronic catarrhal conditions. The latter cir-

cumstance would confirm what seems to be the opinion of
Virchow, that a progressive *irritation* [hyperæmia, inflamma-
tion, no matter how induced], in the part in which an angi-
omatous tumor appears, causes granulation which leads to
the new formation of blood-vessels, and that some of these
becoming isolated, atrophy of intervening tissue is pro-
duced. The original distribution of blood-vessels no doubt
influences the development of the tumor. It is worthy of
remark that in all the cases of intra-laryngeal angioma
known, the growth occurred near the anterior commissure
of the vocal bands, and in all but one (to be sure there are
only five altogether) on the right side. All the cases oc-
curred in the male sex. Heitzmann says we can distinguish
three varieties of angioma, according to the nature and dis-
tribution of the blood-vessels—simple, lobular, and cavern-
ous: Simple angioma is to a great extent composed of
newly formed capillary blood-vessels, between which, in
more or less uniform distribution, is a varying amount of
fibrous or homogeneous connective tissue ; lobular angioma
is composed of coils of large capillary blood-vessels, held
together by delicate fibrous tissue, while between the coils
this tissue is somewhat denser ; cavernous angioma is con-
structed on the plan of cavernous tissue—*i. e.*, composed of
venous sinuses, which lie close to each other, separated only
by walls of fibrous tissue, in which are capillary blood-ves-
sels and sometimes bundles of smooth muscle-fibres, the
cavernous sinuses being filled with blood-corpuscles.

All the five laryngeal angiomata seem to have been of the
cavernous variety.

D.—Symptoms and Diagnosis.

The symptoms of intra-laryngeal angiomata are like those
due to other tumors in the same locality, of the same size,
form, and consistence. In all the cases observed the voice
was affected ; thus in Heinze's case it is noted that the voice

was veiled; in Fauvel's cases there was much hoarseness—forcing one patient to give up his business; and in my cases there was increasing hoarseness in one and huskiness and acantophonia in the other. This was really the only symptom that came from, and pointed to, the tumor; and I have observed as a striking peculiarity that temporary local congestion from any cause made this symptom more prominent; there was no interference with respiration, no cough, no pain, no difficulty of swallowing.

The diagnosis can be made only by means of a laryngoscopic examination. The tumor is then seen. As I have already said, the seat of the tumor was in each instance near the anterior commissure of the vocal bands; in one case it was within the ventricle, in the others on the free border of the vocal band. The attachment was always what is called sessile; even where there was a pedicle this was short, and, in my case, even lengthwise attached. The shape of angiomatous tumors is usually roundish; the size varying from that of a small pea to that of a bean; the surface either smooth or uneven, the former more often; the coloration reddish- or bluish-black, and the consistence semi-hard. The latter point may be determined by the laryngeal sound or probe; also that the tumor is indentable without being fluctuating. In speaking of the diagnosis of his own case of angioma, Mackenzie says[1]: "No person who saw the growth, before its removal, could doubt its being a true angioma, and in judging of its nature, I rely more on the appearances presented by the living growth, than on the subsequent microscopical examination." I agree with Mackenzie that the correct diagnosis may be made by laryngoscopical examination. There is a liability to make a mistake, however, when the tumor is a hæmatoma, a " vascular cyst " (in which case there is usually fluctuation discernible),

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[1] *Op. cit.*, p. 53.

or else what might be called a combination angioma, *i. e.*, angio-myxoma, angio-sarcoma, etc. The microscopical diagnosis of the tumor after removal is histologically confirmatory.

E.—PROGNOSIS.

Although the number of intra-laryngeal angiomata observed is too small to generalize from, the prognosis may be said to be, with proper treatment, generally favorable both as to life and as to voice. But, in making a prognosis, the fact must not be overlooked, that owing to their situation near the anterior commissure of the vocal bands, the treatment of such tumors belongs to the most difficult operations in laryngo-surgery. They have, however, not been observed to be large enough to cause death by suffocation, and the voice seems to have been entirely restored in each case. The danger from hemorrhage after operations will be spoken of under the heading of treatment, and, of course, special circumstances can render the prognosis, as is the case with other tumors in the interior of the larynx, unfavorable. Recurrences have not been observed to take place.

F.—TREATMENT.

The only effective treatment is the removal of the tumor. In cases in which the voice alone is affected, there is no indication for any external cutting operation in order to produce artificial access of air, and the tumor should, if possible, be removed by the aid of the laryngoscope through the mouth and the natural upper orifice of the larynx. Thyrotomy is entirely out of the question, and the three other cutting operations for gaining access to an intra-laryngeal tumor, viz., subhyoidean pharyngotomy, inter-thyro-cricoid laryngotomy, and tracheotomy, are so exceedingly rarely necessary in the cases in question, that I can confine myself to intra-laryngeal methods. These

consist in chemical and mechanical means. The purely chemical embrace caustics and escharotics; the purely mechanical embrace cutting, pinching, tearing, and ligating, while galvano-cautery partakes of the character of both means.

As it is known that an operation upon an angiomatous tumor in any situation may be attended with considerable hemorrhage, it is natural to prefer a method which at the same time that it extirpates the tumor is hæmostatic. On this account Heinze recommends galvano-cautery above all other means. He says: "As to treatment, it may be laid down as the rule, that in order to avoid possible danger, since it cannot beforehand be known whether a tumor of this kind will give rise to severe bleeding or not, the galvano-cautery should be employed for the removal or destruction of laryngeal angiomata."[1] The manner in which he carried out this method was by repeated punctures of the tumor with a sharp-pointed galvano-caustic instrument and cauterization of the entire ventricle (*i. e.*, the place of attachment) with a flat galvano-cautery bent on its side. He had previously removed a portion with an ordinary wire loop; and certainly for ligation the heated galvanic wire cannot be used as safely or as frequently as the simple wire of the écraseur, on account of the injury it might inflict upon the vocal organs on account of the difficulty of limiting the action of the heated platinum loop to just the small point that it touches.

On the subject of caustics and escharotics, I agree with Mackenzie, "that if sufficiently powerful to be effective, they are very likely to cause spasm of the glottis, or to give rise to inflammation of the adjacent mucous membrane."[2] On the other hand, if not sufficiently powerful, they are

[1] *Loc. cit.*, p. 136.
[2] *Op. cit.*, p. 82.

more apt to cause an increase than a diminution of the tumor. When they are to be tried, I would recommend the *careful use* of nitric acid, acid nitrate of mercury, chloride of zinc, or caustic potash, each of which I have successfully employed by means of my applicator in the interior of the larynx in other cases.

Taking it all in all, I think that instrumental removal by " pinching," combined with torsion and evulsion, is the most effective, advantageous, and safe mode of operating upon intra-laryngeal angiomata. This seems to have been the manner in which Fauvel (and Poyet) successfully operated in his two cases, and was the means by which I cured my patients. The best instruments for the purpose are my blunt evulsion forceps. I have some made to open laterally, and some to open forward and backward. The bend which I have given them is the same as that of my applicator, viz., one and a quarter right angles, or, more accurately, 114°. The preparation of the patient and the detailed method of procedure of operating upon intra-laryngeal tumors I have first described nearly twenty years ago.[1] Hemorrhage can be controlled by either the inhalation or injection (or both combined) of a nebulized diluted solution of the persulphate of iron.

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[1] *See* Laryngoscopal Surgery, Illustrated in the Treatment of Morbid Growths within the Larynx. Being the Prize Essay to which the American Medical Association awarded the Gold Medal for 1865, p. 25 *et seq*.

TWO PECULIAR CASES OF TYPHOID FEVER—ONE AT THE AGE OF SIX MONTHS ; ONE BEGINNING WITH PNEUMONIA, AND WITH HEART FAILURE CONSPICUOUS.—EFFECT OF DIGITALIS.

By MARY PUTNAM JACOBI, M.D.

CASE I.—*Typhoid in infant six months old.   Pneumonia at outset.   Recovery.*

The following case occurred in the practice of my friend, Dr. A. S. Daniels.

The most striking peculiarity of this case is the tender age of the patient—six months.   On account of the rarity of typhoid fever at this age, the diagnosis was at 'first much embarrassed ; and the embarrassment was increased by the early onset of cerebral and pulmonary symptoms.   The illness lasted from the 21st of July until the middle of August, the first complete defervescence being observed on the 11th.   The duration was thus three weeks.   Diarrhœa existed for two or three days without fever.   Then, on July 21st, the child had a slight convulsion and chill (?) (according to statement of mother), followed by fever, which persisted till the 24th, when the temperature was found to be 105°, and the pulse 140.   At this time the diarrhœa had ceased, but the child exhibited a combination of cerebral and pulmonary symptoms, such as is not infrequently seen in the pneumonia of children.

Pulmonary Symptoms.

The respirations were eighty-eight in number, and the breathing harsh all over both lungs, while tubular breathing existed at the apex of the right.

Cerebral Symptoms.

The pupils were somewhat contracted ; the conjectiva injected,

and muscles of the neck somewhat rigid, causing some retraction of the head.

The slightest movement caused the child to cry violently ; but when undisturbed, she remained quiet.

A diagnosis was made of pneumonia, with the hyperæmia of the basal meninges, which so often accompanies pneumonia of the apex in young children.   The next morning the temperature had risen to $105\frac{1}{4}°$.   There had been no stool for three days ; a dose of rhubarb and magnesia was given, and in the evening the temperature had fallen to $104°$, and from this date never rose above $104.5°$, indeed only reaching that point once.

The temperature is shown on the accompanying chart (A.), and may be also tabulated as follows :—

July 24th, Evening, 105°.	Aug. 2d, A.M. . . 102°.
" 25th, A.M. . $105\frac{1}{4}°$.	" 3d, A.M. . . 102°.
" " P.M. . . 104°.	" " P.M. . . 103°.
" 26th, A.M. . . $103\frac{3}{4}°$.	" 4th, A.M. . . $101\frac{3}{4}°$.
" " P.M. . . 103°.	" " P.M. . . 102°.
" 27th, A.M. . . 104°.	"· 5th, A.M. . . 101°.
" " P.M. . . 103°.	" " P.M. . . $103\frac{1}{4}°$.
" 28th, A.M. . . 103°.	" 6th, A.M. . . 102°.
" " P.M. . . 104.5°.	" " P.M. . . 103°.
" 29th, A.M. . . $103\frac{1}{4}°$.	" 7th, A.M. . . $101\frac{1}{4}°$.
" " P.M. . . 101°.	" " P.M. . . $101\frac{1}{4}$.
" 30th, A.M. . 102°.	" 8th, A.M. . . 103°.
" " P.M. . . $102\frac{3}{4}°$.	" " P.M. . . 100.5°.
" 31st, A.M. . . 102°.	" 9th, A.M. . . 100.5°.
" " P.M. . . 102°.	" " P.M. . . 99°.
Aug. 1st, A.M. . . 102°.	" 10th, A.M. . 99°.
" " P.M. . . 103°.	" " P.M. . . 100°.

The maximum height of the temperature is reached at the beginning of the period of observation, circumstances different from the typical curve of typhoid.   But the child had been already sick with fever for three days when the observation began, so that the possibility of a more gradual ascent of temperature is not excluded.   This brusque début is paralleled in the annexed temperature curve of a typhoid case described by Cadet de Gassicourt [1] (chart B.).

The first descent, from $105\frac{1}{4}°$ to $104°$, took place in the evening, but after the operation of a purgative.   The subse-

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[1] "Maladies des enfants."   Paris, 1880.

Day	Resp.	Pulse.	Time
JULY 24	88	140	E M
" 25	80	140	E M
" 25	80	140	E M
" 26	60	140	E M
" 26	60	140	E M
" 27	80	140	E M
" 27	80	140	E M
" 28	80	140	E M
" 28	120	160	E M
" 29	100	140	E M
" 29	80	120	E M
" 30	60	120	E M
" 30	60	120	E M
" 31	60	120	E M
" 31	60	124	E M
AUG. 1	60	120	E M
" 2	60	120	E M
" 3	40	120	E M
" 3	40	120	E M
" 4	40	120	E M
" 4	40	120	E M
" 5	40	120	E M
" 5	40	130	E M
" 6	40	130	E M
" 6	40	120	E M
" 7	40	130	E M
" 7	30	120	E M
" 8	40	120	E M
" 8	40	120	E M
" 9	41	120	E M
" 9	36	120	E M
" 10	30	120	E M

CHART A.

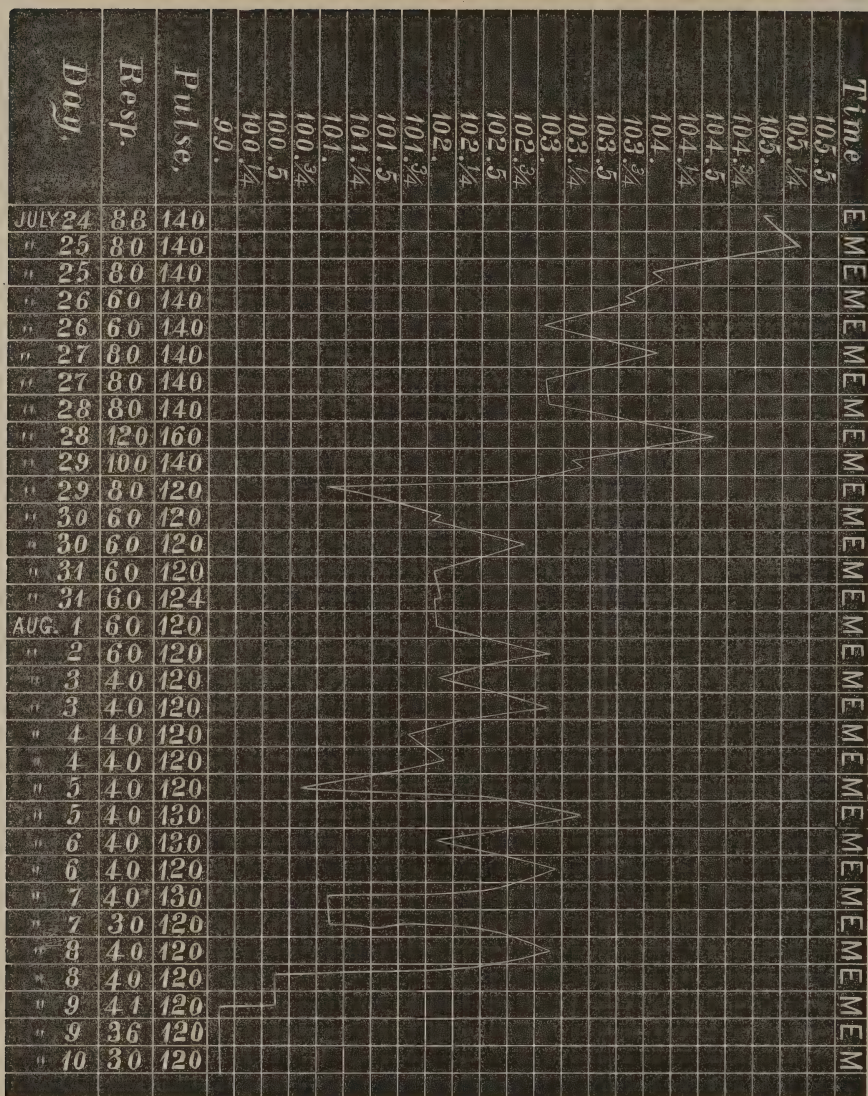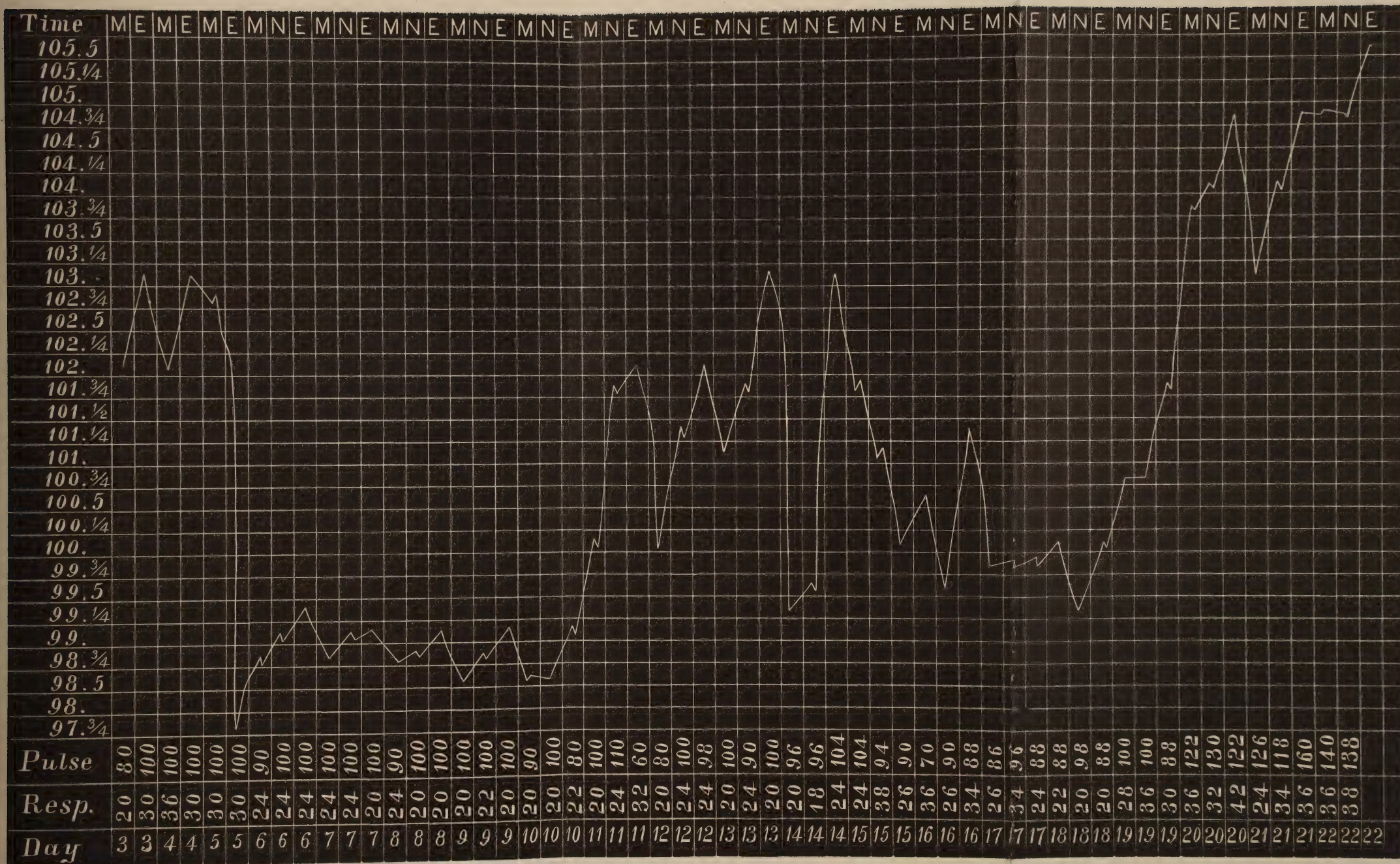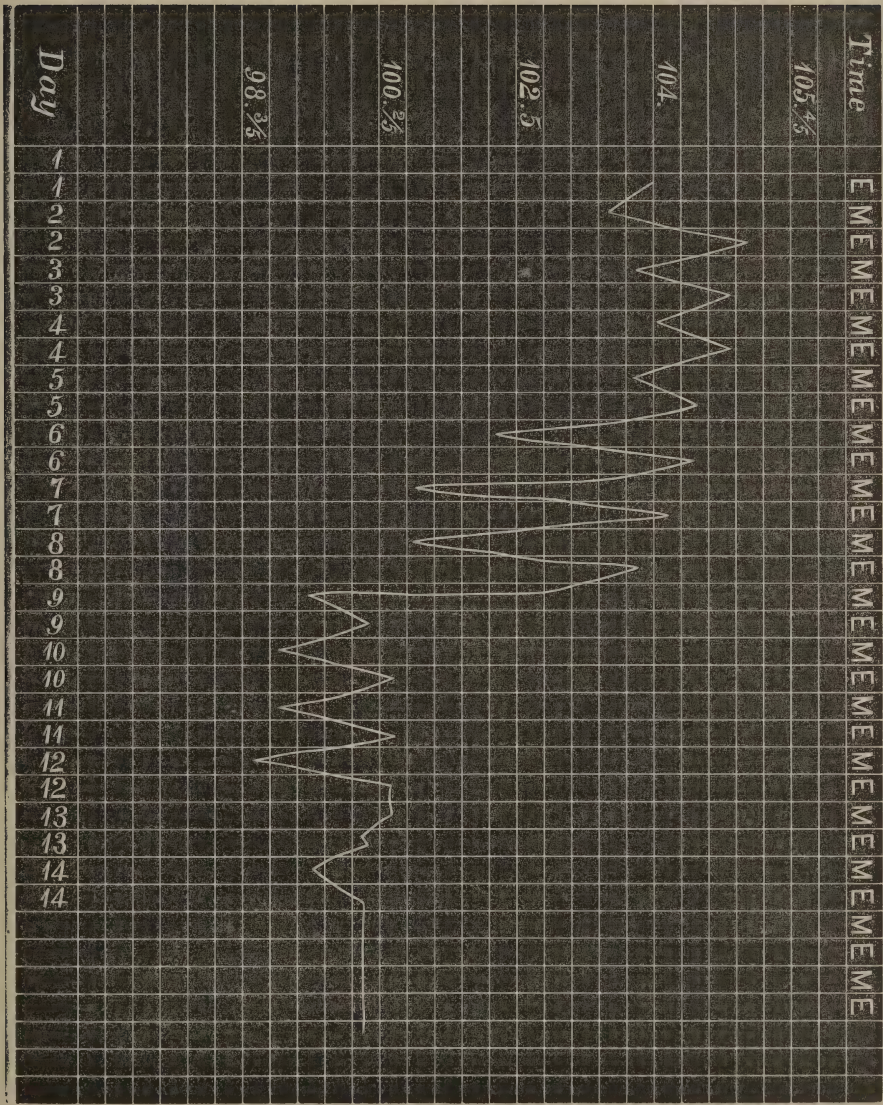| Time	M	E	M	E	M	E	M	E	M	N	E	M	N	E	M	N	E	M	N	E	M	N	E	M	N	E	M	N	E	M	N	E	M	N	E	M	N	E	M	N	E	M	N	E	M	N	E	M	N	E	M	N	E	M	N	E	M	N	E
Pulse	80	80	100	100	100	100	100	90	100	100	100	100	100	90	100	100	100	100	100	90	100	80	100	110	60	80	100	98	100	90	100	96	96	104	104	94	90	70	90	88	86	96	88	88	98	88	100	100	88	122	130	122	126	118	160	140	138
Resp.	20	30	36	30	30	30	24	24	24	24	24	20	24	20	20	20	22	20	20	20	22	20	24	32	20	24	24	20	24	20	20	18	24	24	38	26	36	26	34	26	34	24	22	20	20	28	36	30	36	32	42	24	34	36	36	38
Day	3	3	4	4	5	5	6	6	6	7	7	7	8	8	8	9	9	9	10	10	10	11	11	11	12	12	12	13	13	13	14	14	14	15	15	15	16	16	16	17	17	17	18	18	18	19	19	19	20	20	20	21	21	21	22	22	22

CHART C.

CHART B.

quent oscillations are irregular, the maximum of temperature occurring in the morning, not only on the fifth, but on the sixth, seventh, and ninth days. On the tenth day, however, the temperature rose in the evening nearly to 103°, from a morning temperature of 102° ; and there was a similar evening rise on the twelfth, thirteenth, fourteenth, fifteenth, and sixteenth days of the illness. Three times before the final defervescence the minimum temperature reached 101° or a little below. On the ninth day of the illness (July 29th) the child received grs. v quinine in a suppository ; and the fall of temperature in the evening to 101° may possibly be attributed to that. On the fifteenth day (August 5th) the morning temperature was 100¾°, and no quinine had been taken the preceding evening. The third fall of temperature, which persisted both morning and evening of the seventeenth day, was also spontaneous ; then there was one more rise of temperature to 103°. Quinine was then given by the mouth, grs. iiss ; this was retained, and repeated in six hours. The evening temperature was then 100.5°, and on the following morning the defervescence, which had occurred in lysis, was complete, and maintained.

The sudden high temperature at the outset was evidently associated with the pneumonia which was observed at the very beginning of the symptoms. The classical moment for pneumonia to appear in the course of a typhoid is during the second or third week. But the occurrence of pneumonia in the first week, though exceptional, has been observed sufficiently often to be admitted ; although it necessarily renders the diagnosis for some time doubtful.

Duration of the Pneumonia.

The tubular breathing began to diminish in intensity on the evening of the eighth day (July 28th), or day of the second maximum of temperature (104.5°). On the next day the temperature fell spontaneously, the quinine having been interrupted on account of the vomiting it occasioned ; and on the tenth day (July 30th) the tubular breathing was only heard in occasional whiffs at the apex of the right lung, the breathing elsewhere being nearly normal. Thus the signs of consolidation yielded without being replaced by signs of resolution,—indication that the consolidation had depended upon intense congestion of the lung without exudation. In children, pulmonary congestion frequently occasions a tubular breathing, not to be distinguished from that of a pneumonia with exudation except by the course of the disease. By

the twelfth day all morbid signs had disappeared from the lungs ; the respirations, originally 80, had fallen to 40, yet the fever continued with rather regular oscillations, the exacerbation being always in the evening.

Abdominal Symptoms.

Simultaneously with the subsidence of the pulmonary symptoms appeared an eruption of rose-colored spots, which confirmed the diagnosis of typhoid fever, already suggested by the severe gastro-intestinal symptoms which had existed from the beginning of the illness. This had indeed been ushered in by a choleriform attack of vomiting and purging, succeeded, during the first days of the fever, by constipation. On the fifth day (July 25th) the abdomen was distended and tympanitic, and the child screamed when it was percussed. On the next day the diarrhœa returned, and the child began to vomit every thing taken into the stomach ; the tongue was red, not coated ; the stools thin, yellow, bad smelling. On the tenth day marked tenderness was discovered in the right iliac fossa, and on this day half a dozen rose-colored spots were discovered on the chest. These disappeared on pressure, to return rapidly as pressure was removed. On this same day the temperature began its third distinct rise.

On the eleventh day (Aug. 1st) this eruption began to fade ; three or four spots appeared on the abdomen, which remained two days, then faded, to be replaced by a few others. The diarrhœa and vomiting were finally controlled by opium and bismuth, while quinine was suspended, and, after August 9th (the nineteenth day), did not return.

Splenic dulness was found increased on the eighth day (July 28th).

Course of the Cerebral Symptoms.

The rigidity of the muscles of the neck, with consequent retraction of the head, noticed on the first day of observation, persisted until the 28th (eighth day of illness, fourth of observation), then began to lessen, but did not entirely disappear until August 5th (fifteenth day). On the sixth day the child had a slight convulsion, after which she " lay as if dead," according to the mother's account.

The sparsity of the eruption, the early onset of the pneumonia, the degree of irregularity of the temperature curve, the brevity of the disease, may, of course, suggest the suspi-

cion that the foregoing case was one of pneumonia, with reflex hyperæmia of the cerebral meninges, and a complication of gastro-intestinal catarrh. The diagnosis of typhoid fever is based on :

The coincidence of cerebral, pulmonary, and gastro-intestinal symptoms.

The persistence of the fever after the subsidence of the first two sets of symptoms while the last continued.

The diurnal oscillations of the fever, becoming more regular as the tubular breathing disappeared.

The absence of any signs of resolution, indicating that the tubular breathing had depended exclusively on congestion.

The character of the stools,—thin, yellow, ochrey,—not green, as in ordinary gastro-intestinal catarrh.

The tympanites and abdominal pain, with maximum of severity in right iliac fossa.

The appearance of the eruption on the tenth day, together with enlargement of the spleen.

CASE 2.—*Typhoid fever with sudden beginning. Heart failure. Effect of digitalis.*

The patient was a woman aged forty-five, who, during some years, had been subjected to overwork and severe mental strain.

Début of the Disease.

The onset of the disease was sudden, beginning with a severe chill. This was followed during the night by fever and a pain in the left hypochondrium. The fever persisted continuously during two days. On the third she was seen for the first time by a physician. During these first two days the patient was occasionally out of her mind, but not actively delirious.

When first visited by the attending physician, it was noticed that the intelligence of the patient was already clouded. The facial expression was apathetic ; the patient only spoke in answer to questions, and although the replies were intelligent, the patient was semi-stupid in the intervals of speaking. She complained of nothing. The pupils were somewhat contracted. The tempera-

ture under the tongue was 102° (in the morning) ; the pulse only
80, compressible, weak, at times irregular. The heart-sounds
were remarkably feeble, but there was no valvular murmur. The
respirations were 30. Over both sides of the chest the respiratory
murmur was extremely harsh, more markedly so on the left side.
There were no râles, however, and percussion resonance was
normal. There were no abdominal symptoms ; the urine had a
sp. gr. of 1030, excess of phosphates, no albumen.

First Diagnosis.

The suddenness of the début, the severe chill, hypochondriac
pains, harsh and accelerated respiration, indicated very plausibly
a central pneumonia with peri-pneumonic congestion. The feeble
heart and clouded intelligence, at so early a period of the disease,
was sufficient evidence of an adynamic type of disease.

During the first two days of the fever, the patient had been
taking repeatedly homœopathic tincture of aconite. The ques-
tion suggested itself whether this medicine had not been really
stronger than supposed, and had thus caused the cardiac feeble-
ness noticed on the third day.

Stimulation of the heart was evidently required. The patient,
on moral grounds, absolutely refused to take brandy. The at-
tending physician ordered digitalis in doses of five drops of the
tincture every two hours, as a " cardiac tonic." Ten grains of
quinine was also ordered in two doses, and a diet of milk and
eggs.

Effect of Treatment.

After four doses of digitalis had been taken, the pulse, in the
evening, rose to 100, and became stronger ; the temperature rose
to 103°. The patient was more stupid, lay with her eyes closed,
and frequently muttering. Insomnia and delirium succeeded in
the night. Ten grains of quinine were again given, and the fol-
lowing morning the temperature had fallen to 102° ; the pulse re-
mained at 100, but fairly strong ; the respirations had risen to 36.
On the evening of this, the fourth day, the temperature again rose
to 103°. The pulmonary symptoms had not progressed.

Second Diagnosis.

A probable diagnosis of typhoid fever was made, with determina-
tion of morbid process to the lungs preceding symptoms of intesti-
nal lesion.

First Collapse.

At 7 P.M. of the fifth day, the patient, after two or three hours'

restlessness, passed into a state of moderate collapse. She be-
came quite unconscious ; the face pale, extremities cold. The
temperature fell to 97¾°, the pulse remained at 100, but became
very weak and irregular. The first sound of the heart was feeble.
The respirations fell to 30 ; respiratory murmur continued to be
harsh over both lungs, while numerous liquid subcrepitant râles
were heard. Slight blowing respirations at the upper third of
left lung.

Nature of Collapse.

" Collapse," observed Wunderlich, " is a generalized disturbance
whose immediate anatomical cause is at present unknown, but
which, as a morbid process, may be described in the same way as
fever. It is more frequent in abdominal affections than any
other." [1] " The direct cause of collapse is any that directly de-
presses the force of the heart, that is of ventricular contraction." [2]

Explanations of Collapse.

At the date of the disease on which I first saw the patient,—the
fifth day,—there had been no diarrhœa, and even no abdominal
pain indicating intestinal lesion. There seemed, therefore, no
reason for supposing the existence of an unusually extensive
typhic infiltration as cause of this early collapse.[3] A diagnosis of
typhoid fever seemed probable, on account of the regular oscilla-
tions of temperature, the cerebral symptoms, and the obscurity of
the pulmonary symptoms. Nevertheless, this diagnosis could not
yet be said to be positive, and the entire cortege of symptoms
agreed with the original hypothesis of a central pneumonia of ady-
namic type, with peri-pneumonic congestion and bronchitis. On
this hypothesis the phenomena of collapse might depend on pul-
monary œdema, whose physical sign was the numerous fine liquid
râles, heard all over the chest. The respiration, however, was not
that of pulmonary œdema ; it was neither superficial nor frequent
enough.

By a third explanation this early collapse would be attributed
to the digitalis, which had been taken in the doses mentioned
during forty-eight hours ; five drops of the tincture having been
taken every two hours. The patient had received ℥ i a day, or

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[1] *Archiv der Heilkunde,* 1861. " Der Collaps in fieberhaften Krankheiten."

[2] Ackerman : " Beitrage zur Theorie des Collaps," *Virchow's Arch.,* Bd. xxv.

[3] Griesinger (Die Infections Krankheiten) quotes from Dittrich and Hamer-
nyk (*Prager Vierteljahrs.,* 1846, Bd. x) cases of generalized infiltration of all
the glands of the intestine, from the duodenum to the rectum. Death ensued
with choleriform symptoms.

℥ ii in all, in much divided doses. In ℥ i of the tincture there are 7.44 grains of powdered digitalis.[1] Thus the total amount taken in forty-eight hours was fifteen grains. According to the method of Hirtz, 0.75 or 1.00 of the powder is administered in infusion during a single day, and the administrations continued during four days. This gives from 13 to $13\frac{1}{2}$ grains a day, and a total of 52 to 62 grains in all.[2] The quantity taken by Mrs. A., as compared with this, seemed altogether too small to be responsible for the collapse symptoms.

Another fact seemed to tell against the influence of digitalis in producing the collapse. In digitalis-collapse the pulse becomes slow, while in collapse during fevers from causes associated with the morbid process, the pulse is usually very rapid. A notable feature in the change of pulse which occurred after the first doses of digitalis was, that its frequency, instead of diminishing, increased as it grew stronger, without, however, becoming excessive ; the number of beats was from 80 to 100.

When the collapse set in the pulse remained at 100, but became weak and irregular.

The digitalis was suspended for six hours, and liberal stimulation administered by means of brandy, ether, and camphor. By the following morning (and after the resumption of the digitalis), the patient was much better. Consciousness was restored ; the temperature had risen to $98\frac{3}{4}°$; the pulse, still 100, had become regular and fairly strong ; the respirations, 24. The respiratory murmur was still harsh, but the rales had greatly diminished in number. Blowing respiration was heard over left scapula. As a remedy for the pulmonary congestion present during the collapse, as either cause or consequence (of the heart failure), pilocarpine had been given hypodermically on the preceding evening. It had, however, produced no effect. On this, the morning of the sixth day, a new specimen of pilocarpine was procured from a different druggist, and $\frac{1}{3}$ grain given hypodermically. This was followed by sweating which lasted profusely for two hours. The patient also vomited once or twice. The patient then received brandy, ℥ ss an hour in four doses, together with a potion containing carbonate of ammonia, camphor, and compound spirits of ether. The patient received an abundant

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[1] The tincture is made with 4 oz. to 2 pints or 32 oz. of alcohol,—thus one part to eight. A drachm, therefore, contains $7\frac{1}{2}$ grains ; and five minims, .62 of a grain.

[2] " Nouveau dictionnaire de médecine : art. digitale."

amount of milk and Leube's meat solution. In the evening she seemed much better than at any time during the illness. Temperature 99.5°; pulse 100, strong, and heart-sounds much stronger ; respiration 24.

Second Period.

During the next four days, the temperature, though oscillating upward in the evening, remained within such narrow limits that the maximum was 99¼°, the minimum 98.5°. The morbid sounds in the lungs, instead of increasing, diminished, and the patient seemed to be merely suffering from the prostration consequent on a severe acute illness.

With this apyrexia, and in absence of symptoms pointing to any organic lesion or definite morbid process, it was difficult to avoid believing that convalescence from a limited central pneumonia had been ushered in by the collapse of the fifth day. Two circumstances, however, were inimical to this belief : the pulse remained constantly at 100, though the respirations were between 20 and 24 ; and the intellect was constantly clouded. Thus there was a strong probability that the apyrexia indicated no real convalescence, but an artificial or incidental depression of temperature, while the morbid process with which fever should naturally be associated, was continuing to progress.

Third Period.

On the ninth day, and for the first time, diarrhœa set in and with great severity, greatly increasing the weakness of the patient. On the eighth day, the abdomen had become slightly tympanitic, but there was no pain in the iliac region, and no enlargement of the spleen. The temperature remained depressed. Seven stools occurred in one afternoon, of which five in the course of three hours. At 6 P. M. the patient was found very weak ; the pulse feeble—varying from 80–100 ; first beat of heart scarcely audible. Face pale, and also the extremities cold. The digitalis had been interrupted for six hours. Bismuth and pepsine were given for the diarrhœa.

Convallaria.

At 9 P. M., the partial collapse continuing, the pulse remaining very weak, and frequently irregular,—it was decided to try the effect of convallaria; 10 minims of the fluid extract were given at 9, and repeated at 11. Brandy was continued. At 1 A. M. of the tenth day, the pulse was weaker, intermittent, 80 ; general appearance of patient much worse. Eyes closed, face and extremities cold.

Return to Digitalis.

The digitalis, which had now been interrupted during twelve hours, was resumed in five-drop doses every two hours ; after that, pulse rose from 80–100, became regular and stronger, and continued to improve, until by 6 A. M., it regained the status previous to the collapse. During the tenth day, the diarrhœa was checked, but returned with violence on the eleventh. At noon of this day the temperature rose, for the first time since the fifth day, to 101¾°, and in the evening to 102°, with a pulse of 110. In the lungs could be heard blowing expiration at upper third of right lung ; at apex of left scapula, an occasional whiff of tubular breathing ; and at the base of the left lung, distinct tubular breathing and occasional rather fine subcrepitant rales. During this day opium was added to the bismuth powders, with directions to be taken until the diarrhœa was checked. By an error of the attendant, the opium was given in larger quantities than intended,—so that two grains of the powder were taken in about four hours. The diarrhœa ceased, but during the night a third attack of collapse occurred. After recovery from this, the patient became, for the first time, actively delirious, then semi-comatose and cyanosed. By the gestures and cries of the patient just before she passed into the latter state, she seemed to be suffering from severe abdominal pains. These proved to be due to distention of the bladder by an enormous quantity of urine (36 oz.). When this had been drawn off, the patient became at once tranquil ; the cyanotic tint disappeared from the face, and the pulse became stronger. The urine had no albumen, but excess of urates, and diminution of chlorides.

Typhoid Eruption on Twelfth Day.

On this, the twelfth day, an eruption of roseola was discovered over the abdomen, extremely abundant and characteristic.

During the next three days I did not see the patient. The attending physician continued the same stimulating treatment, with moderate amount of opium, holding the diarrhœa in check. On the morning of the 13th day the digitalis was suspended, and continued suspended during 4½ days, thus till the afternoon of the 17th. During this period the attacks of collapse continued to recur more and more frequently. The range of pulse, temperature, and respiration was as follows. [See also chart c.]

13th day	Temp.	A.M. 101°	Noon 101$\frac{3}{4}$°	P.M. 103°
	Pulse	" 98	" 100	" 90
	Resp.	" 20	" 24	" 24
14th day	Temp.	A.M. 101$\frac{1}{2}$°	Noon 99.5°	P.M. 103°
	Pulse	" 66	" 96	" 104
	Resp.	" —	" —	" 24
15th day	Temp.	A.M. 101.3°	Noon 101°	P.M. 100$\frac{3}{4}$°
	Pulse	" 104	" 94	" 90
	Resp.	" 26	" 38	" 26
16th day	Temp.	A.M. 99$\frac{3}{4}$°	Noon —	P.M. 101$\frac{1}{4}$°
	Pulse	" 96	" —	" 88
	Resp.	" 34	" —	" 34
17th day	Temp.	A.M. 99$\frac{3}{4}$°	Noon —	P.M. 100°
	Pulse	" 86	" —	" 60–50
	Resp.	" 26	" —	" 20 labored.

On this last day the fall of the pulse in the evening had been preceded by two doses of digitalis taken during the afternoon.

The resumption of the digitalis was dictated by two observations : 1st, the improvement of the pulse that followed its administration on the third and also on the ninth day ; 2d, the continuance and even increase of the symptoms of heart failure during the four days and a half that the digitalis was discontinued, and during which liberal stimulation was employed. The diarrhœa was quite moderate. The four non-digitalis days belong to two different temperature periods. On the 13th and 14th the evening temperature was 103°. But the 15th and 16th belong to a second period of antipyrexia. This was ushered in on the evening of the 15th day by an alarming collapse occurring in the afternoon. The entire body was bathed with cold perspiration, and, as was now constantly the case in these "sinking attacks," the face became partially cyanosed. In the evening the temperature was 100$\frac{3}{4}$.° From this date to the 18th inclusive, the temperature ran as follows. [See chart C.]

	15th.	16th.	17th.	18th.
A.M.	101.3°	99$\frac{3}{4}$°	99$\frac{3}{4}$°	100°
P.M.	100.$\frac{3}{4}$°	101$\frac{1}{4}$°	100°	100°

THE RANGE OF PULSE.

	15th.	16th.	17th.	18th.
A.M.	104	96	86	88
P.M.	90	88	60–50	Imperceptible.

On the evening of the eighteenth day, and just at the close of this second low temperature period, the patient, who had had many sinking spells during the day, was at 9 P.M. apparently dying. First sound of the heart inaudible ; the face mottled ;

the lower jaw dropped ; the entire surface of the body covered with a cold perspiration ; the eyes open with slightly contracted pupils ; breathing stertorous.

Musk.

At this time ten grains of musk were inserted into the throat, and a hypodermic syringeful of brandy given—this at 11;—at 11.30 power of swallowing returned, and patient began to rally ; at 12 musk repeated ; at 1 A.M. the whole appearance of the patient was changed. She was sleeping quietly ; the face pale but not mottled ; pulse full but not strong ; the patient assisted in turning herself in the bed. At 2 A.M. musk repeated. The patient recognized her son at 6 P.M. The temperature was 100¾°, pulse 100, and stronger than at any time in forty-eight hours ; first sound of heart returned. The patient continued in this improved condition until 9:30 P.M., when she again became almost unconscious and refusing to swallow. The pulse fell to 80. During the day, owing to some extraneous circumstances the musk was not repeated, so that the patient only took in all, twenty grains. The nineteenth day ushered in the second and final febrile period. The temperature in the morning was 101°, in the evening, 103°, with a pulse of 110. The range of pulse and temperature during next two days was as follows :

19th	Temp.	A.M. 101°	Noon 101¾°	P.M. 103°
	Pulse	" 96	" 100	" 110
	Resp.	" 26	" 34	" 34
20th	Temp.	A.M. 103.5°	Noon 104°	12 P.M. 104¾°
	Pulse	" 122	" 124	" 122
	Resp.	" 36	" 30	" 42
21st	Temp.	A.M. 103°	Noon 104¾°	P.M. 104.5°
	Pulse	" 126	" 122	" 160
	Resp.	" 24	" 30	" 42
22d	Temp.	P.M. 104¾°	Noon 104¾°	Death at
	Pulse	" 124	" 140	2.45 P.M.
	Resp.	—	" 36	

During this second febrile period, cold packs were used for the first time on the 20th day. During this day the pulse and temperature varied frequently. The cold pack repeated every four hours on the evening of the 21st day, the temperature having risen by 8 P.M. to 104.5°, and the pulse to 160, the packs were applied every fifteen minutes for a couple of hours. At 10 P.M. temperature had fallen to 103¾°, and pulse to 112.

During the 21st day the cold-water treatment was continued, but by means of cold sponging. Quinine was given with no evident effect. The attacks of collapse continued to recur, but without

being accompanied by fall of temperature. In one of these, on the afternoon of the 22d day of the illness, the patient suddenly died, with a temperature of 105.5°, a pulse of 140, and respiration at 38.

The points of special interest in the foregoing case are :

1st.—The suddenness of the début of the disease, and early appearance of pulmonary symptoms.

2d.—The amphibolic temperature curve.

3d.—The remarkable predominance of symptoms of heart failure.

4th.—The effect of the digitalis, and the urgent question whether this were in any way responsible for the cardiac collapse.

1st.—The fact that typhoid *may* begin quite suddenly with a chill, is recognized by Griesinger, as also the difficulties of diagnosis to which this exceptional circumstance is liable to give rise. The appreciable physical signs during the first days of the observation (third and fourth days of the illness) were those of pulmonary engorgement. On the fifth day the râles of typhoid bronchitis appeared, though not at the time distinctly recognized as such. This pulmonary engorgement, when considered in connection with the degree of cardiac feebleness which coincided with it from the beginning, was evidently due to this, and indeed the first important result of the heart failure.

2d.—The temperature curve resembled that which Wunderlich describes as " amphibolic,"—consisting of two febrile periods, separated by a prolonged remission of fever. " This remission may even be ushered in by collapse, and usually begins in the middle of the second or commencement of third week. For several days the symptoms are strikingly favorable ; then all at once, there is a relapse. This march of the temperature is often seen in aged persons, or those who have been in ill health previous to the fever." [1]

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[1] Wunderlich : " Medical Thermometry," p. 315.

This double fever curve approximately corresponds to the double period characteristic of all typhoid fever. The first fever period belongs to the characteristic fever process, but may include complications, as pneumonia. Anatomically this period corresponds to the infiltration and commencing ulceration of Peyer's glands. The duration is seldom shorter than fourteen days; never longer than twenty-eight; on an average, seventeen to twenty-one days. The second period corresponds to the regression of the typhoid process, and of the organic alterations caused by it. Anatomically it corresponds to the completed ulceration in the bowel, and also to its cicatrization.

The fever of the first period is typical; that of the second is irregular.[1]

The two periods thus described differ from the amphibolism described by Wunderlich: 1st, in that they are not separated by a febrile remission, as are the two segments of the amphibolic curve; 2d, that they are not peculiar to aged people, or to those in a state of previous ill health; 3d, that the duration of Griesinger's period is rather longer than the first half of Wunderlich's curve.

In Mrs. A.'s case, the first febrile period only lasted five days, then was terminated by the collapse which ushered in the remission of a week's duration. It illustrated the possibility signalized by Griesinger, of pneumonia complication in the first period. But the premature termination of this first febrile period (at five days instead of ten or fourteen) suggests that neither the remission nor the collapse were spontaneous, but the result of artificial intervention.

3d and 4th.—The last two considerations, that of the remarkable predominance of symptoms of heart failure, and the influence of digitalis, may best be taken together.

A certain celebrity attaches to the use of digitalis in

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[1] Griesinger: Die Infections Krankheiten.

typhoid fever, owing to the circumstances of its intro-
duction into practice, and the controversies to which this
has given rise.

In 1814 Clutterbuck praised digitalis as a specific against
fever, and as admirably indicated in typhus.[1]

According to Ferber, Currie compared the effect of digi-
talis with that of venesection, and Rasori used it exten-
sively in his contra-stimulant system of treatment.

In 1860 Hankel of Leipzig reported eighty cases treated
by digitalis,—" 45 mit glucklichem ausgang." [2]

In 1862 Wunderlich published a paper in the same *Ar-
chiv* to define the indications for digitalis in typhoid fever.
He protests against " barbaric statistics " based on entire
series of cases, and denounces as a "crude empiricism " the
attempt to judge of a remedy by the percentage of cures
out of such a series. " A medicine may be entirely im-
proper, though followed by no fatal result, or, on the other
hand, excellent, although many fatal cases may occur
among those in which it is given " (*loc. cit.*, p. 111).

The next series of observations were made by Ferber
and published in 1864 (*loc. cit.*).

In 1865 Thomas published a new series.[3]

Hirtz, of Strasbourg, first introduced into France the sys-
tematic treatment of typhoid by digitalis.[4] His method,
adapted and somewhat modified from the Germans, has
been recently systematically elucidated and defended by
his pupil, Bernheim, at Nancy,[5] in a series of clinical lec-
tures that compose an interesting monograph on the subject.

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[1] Dictionnaire en 30 volumes. Quoted by Ferber in *Virch. Arch.*, Bd. xxx.,
1864.

[2] *Archiv der Heilkunde*, 1860. It is not clear what the author means by this
expression. If that only forty-five out of eighty cases recovered, the success of
the treatment could hardly be considered brilliant. But from the context, this
does not seem the intention.

[3] *Archiv der Heilkunde*, Bd. vi.

[4] *Bulletin de thérap.*, 1862, and " Nouveau dictionnaire de médecine pra-
tique," art., digitale, 1869.

[5] *Clinique médicale*, 1877.

In Great Britain, Dr. Grimshaw introduced the subject in a communication to a medical society in Dublin, and subsequently reported in the *Dublin Quarterly* for June, 1873. The cases, thirteen in number, in which the digitalis was given, were all of typhus, not typhoid fever. The author based his practice on the strong recommendation of Fothergill of digitalis in "acute asthenic disease," and also on the experiments of " Brunton, Hanfield Jones, Dybkowsky, Pelikan, Hilton, Herzog, Valentin, Stevenson, Traube, Von Bezold, who have definitely established the fact that digitalis increases the force of ventricular contraction."

In the number of the *Practitioner* [1873] which followed the publication of Grimshaw's paper, Anstie commented at length upon it. He declared that the report of Dr. Grimshaw's cases " should dissipate the bugbear of heart depression as a symptom of digitalis treatment." He himself, however, contributed no new cases to the elucidation of the problem.

On the authority of Grimshaw's report, Anstie's comment on it, and a review of Hankel's cases in the *British and Foreign Med.-Chir. Review*, xxvi., 513, Wood declares: " Digitalis is often of great value in various acute diseases, such as adynamic pneumonia and adynamic fevers, by maintaining the heart's action. It can have no effect on the diseases themselves, but may help most opportunely to sustain the heart during a crisis or a period of strain upon it." [1]

Digitalis was originally used in typhoid fever as an antipyretic, and its value measured principally by its effect in reducing temperature. Hankel, however (*loc. cit.*), praised digitalis as a remedy for calming delirium and, indeed, all the nervous symptoms of the disease, but considered an ex-

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[1] " Treatise on Therapeutics," 1880. 3d edition, p. 145.

cess of temperature, associated with rapid and weak pulse, to be the main indication for the use of the drug.

Hirtz (*loc. cit.*), in formulating his digitalis method, declares that it is only applicable to the first period of typhoid fever, which, in accord with Griesinger, he recognizes as indicating the primary evolution of the typhic process. Given at this time, digitalis will, in from twenty-four to thirty-six hours, depress the temperature often below the norm. In the majority of cases the temperature begins to rise again in twelve hours, but in a certain number the defervescence is final. Out of fifty-three cases treated with digitalis by Bernheim, terminal defervescence occurred in six.

1st case.—Defervescence third day from first dose, and after total ingestion of 1.50 grammes in infusion (22 grains).

2d case.—Defervescence began on ninth day; complete on seventeenth.

3d case.—Defervescence began on tenth; complete on sixteenth.

4th case.—Defervescence began on thirteenth; complete on seventeenth.

5th case.—Defervescence began on twelfth day after first dose of digitalis; finished the twenty-second.

6th case.—Defervescence began on fourteenth day; finished the twenty-second.

In the forty-seven other observations, the fever returned and continued, notwithstanding the continuance of the digitalis.

As bearing on our case, the influence of digitalis on the temperature is chiefly of importance in estimating how far the defervescence of the fifth day was due to it. The digitalis was originally given by the attending physician on the third day of the illness, with no view to an antipyretic effect, for the temperature of the patient was quite moder-

ate (102° F.). It was directed against the cardiac weakness indicated by a pulse feeble and rather slow for the temperature (80), and also by the marked diminution in the distinctness of the first sound of the heart. For the purpose of a cardiac tonic, the digitalis was given in small doses (five drops every two hours); thus (see note to p. 39), 7.44 grains in twenty-four hours.

The antipyretic method has always employed the infusion of digitalis, either in substance or in powder,—the infusion given in tablespoonful doses every hour. The following amounts have been given:

Hankel.—1.25 grammes in twenty-four hours; continue from three to six days. Total, 3.75 grammes (56 grains) to 7.50 grammes (112 grains).

Traube.—3.75 grains an hour until pulse and temperature lowered.

Thomas.—26 to 30 grains in twenty-four hours.

Ferber.—180 to 210 grains in from twenty-four hours to eight days.

Wunderlich.—10 to 20 grains a day for three to six days (30 or 60 to 60 or 120 grains in all); cease with the decided diminution of pulse.

Grimshaw.— ℥ i to ℥ iss. infusion every three hours, thus ℥ viii to ℥ xii in twenty-four hours, containing 53 to 80 grains. This in *typhus* fever.

Hirtz.—5.00 grammes to 10.00 grammes in twenty-four hours (75 to 150 grains) produce defervescence promptly, *and yet are less dangerous than small doses continued for a long time.* Habitual dose: .75, 1.00, or 1.50 during twenty-four hours (11, 15, or 22 grains); this continued until an apyretic effect is begun, then cease. Usually three to four days, or total of 33 to 88 grains.

Bernheim.—Identical with method of Hirtz.

Comparing the doses given in the foregoing clinical ex-

periments with those administered in our case, we should infer, if guided by statistical analogy, that the amount of digitalis received by Mrs. A. previous to the defervescence of the fifth day had been altogether too small to be directly responsible for this. But, individualizing the circumstances as much as possible, we must ask:

1st.—Whether the digitalis in form of tincture would be more depressing than in form of infusion, and thus act more powerfully at a smaller dose and after a shorter time.

2d.—Whether the mode of action of digitalis, as an antipyretic, be such that it is intensified in persons with feeble heart.

3d.—Whether the defervescence observed in this case were not a phenomenon of cardiac collapse, rather than the collapse an element of a rapid defervescence; and whether this collapse were due to the direct action of the infectious agent, or to the direct and unusual action of the digitalis on the heart, apart from any antipyretic influence.[1]

The first question may be promptly answered in the negative. The tincture is much less liable to be depressing than the infusion; on this account it is habitually used in the treatment of apyretic heart diseases, and on this account was selected for Mrs. A.'s case. The method of divided doses employed (five drops every two hours) is identical with that used by the authorities employing the infusion (spoonful from hour to hour).

The second question, however, opens up a wide field of inquiry. By what mechanism does digitalis depress the febrile temperatures? Is this depression secondary to that of the circulation, and is the drug indicated as an antipyretic only when the heart is strong enough to bear this; or is it,

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[1] Ackerman: Action of Tartar Emetic, contribution to theory of collapse, *Virchow's Archiv*, Bd. xxv., 1862. "Collapse caused by tartar emetic is due to the directly depressing effect this poison exerts on ventricular contraction." The author infers that such cardiac depression is the primary phenomenon in all collapse.

on the contrary, particularly indicated when the heart is weak? If the latter be true, is digitalis really indicated as a cardiac tonic in fevers as in apyretic morbid states? Or is the suggestion for such use based on a fallacious analogy, and dangerously misleading, because failing to take into account certain cardiac conditions peculiar to the fever, and not present in apyretic forms of cardiac disease?

Wood observes that "there is no physiological basis for the antipyretic theory of digitalis," because it does not depress the temperature below the normal in apyretic states. But the same may be said of quinine and salicylic acid, even of the cold bath,—in fact of all antipyretic agents which act in fever, but either not at all, or only very slightly, in health, or in non-febrile diseases. A drachm of the tincture in twenty-four hours is frequently given in organic heart diseases (ten to twenty drops ter die being the officinal dose[2]), and for a long time, without any change in temperature being observed. The rapidity of the pulse is lowered, the force of ventricular contraction indubitably increased,—yet not the least sign of collapse is observed.

In the cases recorded by the authors we quoted, when a fall of temperature is effected by digitalis, it is always accompanied by fall of pulse, while in some cases the pulse falls, the temperature remaining the same. In forty-two cases observed by Ferber, the pulse was affected first; in the others, simultaneously with the temperature. In ten cases recorded by Wunderlich, the pulse fall was as follows (p. 52):

From Hankel we note the following cases:

Case.—End of second week. Pulse 132. Digitalis given (1.25 grm. a day). On following days the pulse ranged, respectively, 128, 116, 116, 108, then became irregular. Digitalis was stopped, stimulants given, yet the pulse continued to fall, and on the next three days marked 96, 60, 68 beats. It then became irregular.

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[2] The same amount as given in Mrs. A.'s case, but in different distribution.

1st case. (4th day of fever).	30 grains in 36 hours.	Fall from 108 to 52.
2d case. 3d day.	30 " " 24 "	" " 102 " 72.
3d case. 4th day.	45 " " 12 "	" " 120 " 72.
4th case. 3d day.	45 " " 12 "	" " 120 " 68.
5th case. 4th day.	50 " " 24 "	" " 132 " 78.
6th case. 4th day.	50 " " 12 "	" " 108 " 60.
7th case. 3d day.	60 " " 36 "	" " 110 " 50.
8th case. 5th day.	65 " " 24 "	" " 92 " 60.
9th case. 6th day.	80 " " 12 "	" " 113 " 70.
10th case. 5th day.	80 " " 12 "	" " 108 " 70.

In the following case the record of the temperature is also given. Digitalis treatment was begun at the beginning of the fourth week:

	TEMP.	PULSE.	
A.M.	40.8° Cent.	112	————
P.M.	40.8°	104	————
A.M.	40.4°	106	————
P.M.	40.6°	112–132	————
A.M.	40.4°	108	Fierce delirium.
P.M.	40.1°	108	Soft.
A.M.	40.0°	84	Regular.
P.M.	40.0°	100	Cease digitalis.

4.5 grms. taken altogether.

Thomas has sometimes observed a fall of pulse before a fall of temperature. This physician did not consider a moderate frequency of the pulse any contra-indication to the use of digitalis. In seven cases the modification was as follows:

Case 1.—Pulse fell from 78 to 38 in 24 hours.
  " 2.   "   "   " 84 " 52 " 12   "
  " 5.   "   "   " 112 " 76 " 12   "
  " 6.   "   "   " 88 " 48 " 36   "
  " 7.   "   "   " 108 " 66 " 12   "

The two following cases detailed by Bernheim (*loc. cit.*)

are interesting for our purpose ; for in both, heart failure was conspicuous :

CASE 1.—Third day. Tem. 40° Cent. (104°), P. 140, R. 36. Pulse dicrotic, with feeble tension ; diarrhœa.

Ninth day, catarrhal pneumonia right lung ; agitation ; 38.7° (101.4°), P. 140, R. 38. Then infus. digitalis 0.75 grms. (about 12 grains).

Tenth day, Tem. 39° (102.2°), P. 128, R. 40. Thus pulse fell, temperature rose.

Eleventh day, somnolence ; sub-delirium ; sweats ; fuliginosities on lips and teeth ; 38.6° (101.4°), P. 104, R. 48. Continued fall of pulse, and fall of temperature, in 48 hours ; rise of respiration.

Twelfth day, tem. 38.9° (102°), P. 80.

Thirteeenth day, tem. 37.8° (100°), P. 64, R. 36. Suppression of digitalis, thus after four potions containing in all forty-eight grms. Tension of pulse increased ; less dicrotism.

Effect on both pulse and temperature maintained during three days (13th, 14th, 15th) ; on the 16th day temperature began to rise, but pulse remained the same, at 70 or 80. Thus the effect on the pulse was produced first, and persisted the longest. By the 20th day the heart's action began to be not only slow but feeble, the prostration and cerebral symptoms increased, and the face began to be cyanosed. Upon this symptom of cardiac failure, the administration of digitalis was resumed in doses of 1.00 (15 grains) per diem.

During two days pulse remained the same.

On third day pulse fell to 64, temperature to 37.2° (98.9°). Suppression of digitalis.

On fourth day pulse irregular, somnolence and delirium persisting. Not until six days after cessation of second dose of digitalis (twenty-eighth day of illness) did pulse rise to 80, with a temperature of 38° (100°), and a marked amelioration was perceptible. Terminal defervescence on thirtieth day.

CASE 2.—Tenth day, tem. 40° (104°) A.M., 40.6° (105.08°) P.M. Pulse 128 A.M., 124 P.M. The first potion of digitalis (0.75) was given on this day, thus a considerable portion between the morning and evening measurements. It failed to affect the temperature, which rose over a degree, but slightly lowered the pulse,—at all events kept it from rising in proportion to the temperature. During the next three days digitalis continued, at same daily dose. On fourth

day tem. 38° (100.4°), P. 108. Patient in stupor. Sounds of heart irregular. Digitalis suppressed ; stimulants given ; but in six hours collapse with vomiting, and fall of pulse to 84, tem. 37.8° (100°). After this the temperature rose moderately during three days, and final defervescence set in on the fifth.

Thus, in the great majority of cases, a fall of temperature caused by digitalis, is accompanied or even preceded by a fall of the pulse rate. The pulse is more readily, more profoundly, and more permanently affected than the temperature.

" The fall of the pulse rate is habitually accompanied by an increase of arterial tension. That is to say, that the work of the heart is increased ; that this organ, debilitated by the fever, is re-enforced in its contractility by digitalis, and perhaps opposes more resistance to the febrile combustion ; * * * pulmonary hypostasis and œdema are less to be feared." (Bernheim, *loc cit.*, p. 434.)

A fall of temperature occurring in typhoid fever treated by digitalis, though occurring in the first week, should not be attributed to the drug if, at the time, the pulse remain unchanged in frequency. Collapsing defervescence may occur spontaneously during the primary period of typhoid, and is always ominous (Griesinger). In spontaneous collapse, the pulse will be feeble, often irregular, small, thready, extremely frequent, or unusually slow.[1] In digitalis collapse, the pulse is slow, not necessarily below the norm., but much below the febrile rate previously maintained. This is shown in the foregoing illustrations, also in the following fatal case from Ferber, where, as the author observes, " the remedy was certainly not free from responsibility for the unfortunate issue " (*loc. cit.*).

Case six of second series.—Girl of sixteen ; received infu-

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[1] Der Collaps in fieberhaften Krankheiten. Wunderlich.—*Archiv der Heilkunde*, 1861.

sion of digitalis, ℥ i. to ℥ vi. water, on eighth, tenth, and twelfth days of illness. In the three days took 210 grains.

On sixth day (before digitalis).	Evening.	.39.2° cent.	P. 108	R. 40
7th.	Morning.	.39.7°	" 112	" 38
8th.	Morning.	.39.25°	" 114	" 32
	Evening.	.40°	" 108	" 32
9th.	Morning.	.39°	" 100	" 36
	Evening.	.40°	" 198	" 28
10th.	Morning.	.38.7°	" 66	" 36
	Evening.	.40°	" 88	" 24
11th.	Morning.	.38.5°	" 64	" 26
12th.	Evening.	.38.25°	" 88	" 24
13th.	Morning.	.37.5° *Pulseless ; cyanosis.* cold extremities ; different cardiac contractions separated by long intervals ; neither vomiting nor sweating.		

13th., early morning, sharp chill and sudden death.

In this case the pulse fell with the temperature, but for neither was the fall more marked than is habitual in the most favorable cases; and this notwithstanding the enormous dose of digitalis administered.[1]

Usually, therefore, digitalis may be expected to act on the pulse rate in fever, as in apyrexia, and reduce its frequency. "In grave cases, however, where the evolution of the disease advanced, and the frequency and weakness of the pulse indicate degeneration of the cardiac fibre, digitalis may lower the temperature, and the pulse remain frequent." (Bernheim, *loc. cit.*)

Did the case of Mrs. A. fall under this category? Certainly not, if the cardiac degeneration which opposed resistance to the characteristic influence of digitalis must be traced to the febrile process. This had not lasted nearly long enough to cause such degeneration. Moreover, it could not be said—at least on the fifth day—that the pulse remained at 100 because it resisted the action of digitalis.

Originally 80 (on the third day) and very weak, it

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[1] These doses probably indicate an inferior quality of the herb employed.

rose to 100 in a few hours after the administration of the digitalis, and at the same time became much stronger. In this state it remained until the moment of collapse, when it became very feeble again, but neither gained nor lost in frequency.

As I never saw the patient before the fifth day of the fever, and as the attending physician had never seen her previous to the third day, there is no observation of the state either of the pulse or heart anterior to her acute illness. It could only be surmised that a patient of forty-five, whose pulse, with a temperature of 102°, marked only 80, and this during the first days of the fever, thus long before any febrile degeneration of the cardiac muscle could have occurred, had, habitually, an unusually slow pulse. For the same reasons, it was to be inferred that the indistinctness of the first sound of the heart, noticed at the outset of the fever, had preceded this. A slow, feeble pulse, with indistinct first sound of the heart without valvular murmur, points to nutritive alteration of the heart-walls, tending toward, or even consisting in, fatty degeneration. In this case, the patient would have entered upon the typhoid fever with a lesion identical in effect, though not in precise nature, with one most to be dreaded during the second period of the disease, but then due to the prolonged influence of the infectious agent and of high temperatures; that is, degeneration of the heart muscle.

In this condition there is certainly reason to fear that the molecular nutrition of the heart, already impaired, will be ravaged with exceptional facility by the injurious influences of the fever, which (both infectious poison and abnormal heat) tend to prevent the movement of assimilation in muscular fibre, by accelerating that of disassimilation.

How should digitalis act in such a case?

Theoretically, this important question must be answered

by reference to two factors: the action of digitalis in apyretic cases of fatty or dilated heart ; the relation of its antipyretic action to its pulse action. Empirically, the results of digitalis treatment of fever must be compared, in cases with and without symptoms of primary cardiac degeneration, and comparison also made between those cases where the medicine is given in the second period of the fever, when the heart is liable to be degenerated, and those where the treatment is adopted during the first period, with an intact cardiac nutrition.

Grimshaw particularly praised digitalis in the continued fever (typhus) where marked cardiac weakness existed in the beginning, or even when fatty degeneration preceded the disease. Thus in his

CASE 2.—Typhus in sailor, occurring just after recovery from a prolonged illness. The heart was found very weak on the sixth day. From the sixth to the fourteenth day the patient received infusion of digitalis, at first ℨ ss, then ℨ ij every third hour. The temperature continued to range from 105° to 103°, but the pulse tension was quickly restored, and the heart sounds gained in strength. The number of pulse beats is not recorded.

CASE 3.—Eighth day, " heart sounds of fœtal character " ; Pulse 136, low tension ; 105.2°. Infus. digitalis ℨ i every third hour.

From eighth to eleventh day, tem. varied from 105° to 103° then fell. Pulse fell on ninth day (twenty-four hours after digitalis) to 120. On eleventh day digitalis increased to ℨ iss ; convalescence on twenty-first day.

CASE 8.—Woman, æt. forty-nine ; had had thirteen children. Pulse small and weak, first sound indistinct ; much mental anxiety. On sixth day ℨ i infusion every third hour.

On eighth day first sound of heart almost absent. ℨ i whiskey given daily.

On ninth day ℨ i liquor strychniæ added to each dose of digitalis, and latter given every second hour.

On twelfth day heart sounds failed.

Thirteenth day, " pulse tracing that of fatty heart ; tends to squaring of apex."

The further details are not given, nor, as seen, is the record of the pulse, complete, but the patient recovered, and the author considered the case a valuable illustration of the power of digitalis as a vascular stimulant. The temperature was never above 102°, a circumstance which he attributes to the fatty heart.

Dr. Grimshaw concludes from his own experience, that digitalis has no influence on the temperature; but is very valuable to prevent or diminish delirium (dependent on imperfect circulation of the brain), to improve the tension and slightly diminish the frequency of the pulse, to increase the power of ventricular contraction, and prevent the loss of vascular tension.[1]

In one of Bernheim's cases, already quoted, the patient began to be cyanosed after interruption to the digitalis: On this symptom of heart-failure, the digitalis was resumed, and the cyanosis disappeared.

Out of one hundred and fifty of Bernheim's cases (cardiac?) collapse was only observed three times. It was always readily dissipated by stimulants. The patients in whom it was observed had taken 0.75 on three successive days, or 36 grains in all. Thus, the dose was rather below that which was borne, in the great majority of cases, with perfect impunity. " Digitalis is quite inoffensive unless the heart is degenerated by the disease, but must be suspended at first sign of collapse." "When integrity of cardiac muscle is suspected, renounce digitalis, or associate it with cinchona or rum " (Bernheim, *loc sit.*, pp. 357, 358). Nevertheless, Wunderlich, like Grimshaw, notices the especially beneficial effect of digitalis " when there is danger of cardiac exhaustion " (*loc cit.*).

" The dicrotism of the pulse is not altered; but a small and miserable pulse becomes persistently fuller and stronger.

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[1] *Dublin Quarterly*, 1873.

The main indication for digitalis in typhoid is excess of temperature, with rapid and weak pulse, and delirium. It is absolutely free from danger " (Hankel, *loc cit.*).

Hirtz declares that the first period of typhoid fever, dependent on the primary typhic process, is alone suitable for the digitalis treatment. That the latter is of no use in the secondary fever of ulceration and absorption, and that digitalis is contra-indicated in cases of moderate fever and adynamic type. But he is only considering the drug as an antipyretic; and lays all the more stress on this action, because he attributes almost all the dangers of typhoid fever to the excess of temperature.

The apparent contradictions in the foregoing statements, which could be multiplied by multiplying authorities, may be reconciled by the consideration that there are at least two forms of cardiac weakness, both manifested by feebleness of the ventricular systole. In the first, the cardiac muscle contracts feebly because its structure is altered, either by the vitreous degeneration, due to the febrile process, or by a fatty degeneration, or even through simple anæmia, which may have preceded this. In the second case, the feebleness is due to deficient innervation, when the excited motor ganglia are overwhelmed by the typhic poison, or exhausted by the long-continued super-stimulus of high temperatures. In either case, the feeble pulse may be either excessively frequent, or abnormally slow. Slowness of pulse may, at any time, be caused when the excited motor ganglia of the heart are overwhelmed by a poison in the circulation, and thus is not infrequent in diphtheria. It seems, however, to be very much less frequent in typhoid, where the feeble pulse is habitually also very rapid,—except in degenerated heart.

In one of Grimshaw's cases, the radial pulse, on the thirteenth day of the fever, yielded a tracing resembling

that of fatty heart.   This was obtained, however, after the patient had been for seven days under the influence of very large doses of digitalis, and the question suggests itself whether the excessive cardiac feebleness were not due to this.

The mass of testimony offered by physiological observations supports two parts at least of the theory of Traube concerning the action of digitalis on the heart. This drug stimulates the inhibitory apparatus, the vagus, and intra-cardiac ganglia,—as may be shown by the increased susceptibility of the vagi to faradization after its administration, and by its failure to act after this apparatus has been paralyzed by atropine.[1]

But in addition, in apyretic states, the digitalis directly stimulates the contractility of the cardiac muscle. In a frog poisoned by woorara, when voluntary movements have disappeared, and the movements of the heart become more and more feeble, injection of a certain dose of digitaline will be followed by an increase in their vigor.[2] Boehm has measured the work done by the frog's heart isolated from its nerves, when digitalized serum is passed through it, and has found that this is increased,—with a certain dose,— while the number of beats is diminished. With large doses the heart is arrested in diastole. Vulpian found that large doses paralyzed the cardiac muscle, causing irregular and incomplete contractions of both auricles and ventricles.[3]

It is this second action of digitalis which is liable to fail in febrile diseases, on account of the degeneration of the cardiac muscle, which can no longer, therefore, respond to the stimulus. The case resembles the apyretic heart diseases in which the heart fibre has become fatty. So long

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[1] Boehm: Ueber den Herzen Gift. Ackerman: *Sammlung klinischer Vorträge*, No. 48, 1872. We have ourselves repeated these experiments, arriving at the same results.

[2] Gourvat : Thèse Inaug., 1871 (quoted by Bernheim, *loc. cit.*, p. 278).

[3] *Mem. Soc. Biol.*, 1865.

as the inhibitory ganglia remain intact, the pulse may still be slowed by the digitalis; but neither its force nor the arterial tension will be increased.

Theoretical Conclusion.

These data accord with the clinical experience that digitalis is predominantly and almost exclusively useful in the first period of typhoid fever,—that is, when the heart muscle and cardiac nerves are intact.

The slackening of the pulse is useful only in so far as it is associated with increased force of the ventricular systole, with prolongation of the diastole, higher vascular tension, and more vigorous capillary circulation. The prolongation of the diastole is the circumstance the most important for the nutrition of the heart itself, since it is during the diastolic pause, or moment of cardiac rest, that molecular assimilation is carried on in the cardiac muscle.

In Mrs. A's case the administration of digitalis was followed by a rise of pulse frequency from 80 to 100, while at the same time the beats increased markedly in strength. This was observed very distinctly on the third, and also on the tenth day of the illness, and is contrary to the usual effect expected. How may it be explained? A slight acceleration of the pulse, preliminary to a fall, has sometimes been observed in physiological experiments with a weak dose of digitalis.[1] But then the pulse becomes at the same time weaker instead of stronger; whereas, in Mrs. A.'s case, the increase of strength was the most important result.

Thomas (*loc. cit.*) observes that occasionally,—just when the pulse is markedly depressed by digitalis, even to the norm,—a sudden increase of frequency occurs. He explains this by reference to the fact that during the digitalis narcosis influences continue to exist tending to accel-

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[1] Hirtz : *Bull. de thérap.*, 1862.

erate the heart's action, and which, from time to time, may overpower the effect of the digitalis.

When the pulse is rapid and feeble in apyretic heart disease, *e. g.* mitral insufficiency, the rapidity is due to the lowered tension of the arterial system. The nutrition of the heart is enfeebled by the shortness of the diastole, which permits too short a period for nutritive assimilation; and the ill-nourished cardiac muscle contracts, therefore, feebly. Prolongation of the diastole by means of digitalis meets, therefore, all the necessities of the case, so long as the integrity of the peripheric vessels is preserved, and as the lowered arterial tension is itself, by a vicious circle, due to the enfeebled action of the heart.

The constant influences tending to modify the pulse during fever are quite different. Apart from the increased temperature, the infectious agent stimulates the excito-motor ganglia at moderate doses, or when their vital resistance is strong; depresses and tends to paralyze them in the converse case.

"In typhoid fever the pulse is not always proportioned to the temperature, which may be very high, while the pulse remains normal. Very severe cases, with 40 and 41 degrees of temperature ($104°$ and $105.8°$) may run their entire course without the pulse reaching 100. In one of my observations the temperature rose to $41.5°$ ($106.7°$) when the pulse was 72, and another where it stood at $41°$ ($105.8°$) when the pulse was 68." [1]

"There are cases where throughout the illness the pulse is 60 or 80, notwithstanding considerable elevation of temperature." [2]

"Does this relatively low rate of pulse in abdominal typhus depend on the gradual rise of the temperature, or must we admit a depressing action on the pulse of the typhic

[1] Bernheim, *loc. cit.*, p. 339.
[2] Griesinger : "Die Infections Krankheiten."

poison? The latter seems probable from the fact that in apyretic forms, or with moderate fever, the frequency of the pulse is often below normal." [1]

The following combinations of circumstances may occur:

Pulse Accelerated (*moderately*).

I.—Inhibitory and excito-motor ganglia both intact; average dose of fever poison; stimulation of excito-motor ganglia beyond average restraint of inhibitory.

Pulse Accelerated Excessively, and Feeble.

II.—Paralysis inhibitory of ganglia by fever poison and temperature; low arterial tension adding mechanical influence.

Pulse Slowed and Feeble (*only out of Proportion to Temperature*).

III.—Excessive initial dose of fever poison.

IV.—Anterior imperfection in nutrition of excito-motor cardiac ganglia or of cardiac muscle; hence failure to respond to febrile stimulus, while nutrition is directly depressed by fever poison.

Pulse Slowed Below Norm.

V.—Overwhelming dose of fever poison; generalized paresis of cardiac nerves, both excito-motor and inhibitory.

The cases in which digitalis is usually recommended in fevers come under class I. Mrs. A.'s case belonged either to class III or IV. We think the action of digitalis in her case (during the initial period of its administration) can only be explained by the following hypothesis: The inhibitory nerve of the heart responded to the stimulus, and the diastole was prolonged, thus facilitating an improved nutrition of the heart muscle. At the same time this muscle responded to the direct functional stimulus of the digitalis; and under the double influence the force of ventricular contraction improved. A simultaneous improvement in the nutrition of the intracardiac ganglia rendered them more

[1] Liebermeister: *Ziemssen's Handbuch*, Bd. iv., Th. 1.

susceptible to the febrile stimulus, and thus the number of nerve discharges or impulses to contraction were increased. The influence of the stimulated inhibitory apparatus, which was inadequate to restrain the number of discharges from the excito-motor ganglia, was manifested in shortening their duration; in other words, the diastole was prolonged at the expense of the systole.

Sphygmographic observations would have demonstrated whether such modification in the relative duration of the two periods of the heart-beat, really took place. It was inferred, because the force of the heart was indubitably increased, and there is not the slightest reason to suppose that the action of the heart on the cardiac muscle can be exercised without its simultaneous and most characteristic action on the inhibitory apparatus.

The following schema illustrates this hypothesis, which, indeed, is scarcely more than the statement of the facts observed. Since the number of beats before and after digitalis was respectively 80 and 100, they stood to each other in the relation of 8 to 10.

Before digitalis. Pulse 80, feeble. Systole and diastole equal.
| S.   D. | S.   D. | S.   D. | S.   D. | S.   D. | S.   D. | S.   D. | S.   D. |

After digitalis. Pulse 100. Diastole longer than systole.
| S.   D. | S.   D. | S.   D. | S.   D. | S.   D. | S.   D. | S.   D. | S.   D. |

During the first two days of digitalis treatment, the effect on the heart was unquestionably beneficial. Nor, as has already been pointed out (pp. 38, 39), could the collapse of the fifth day be attributed to the direct action of digitalis on the heart, because in digitalis collapse the pulse always becomes abnormally slow, and the slowing of the pulse precedes the collapse by a period of hours or even days,—whereas, in Mrs. A's case, the pulse remained at 100, though becoming feeble and even irregular.

The feeble pulse in the crisis of the fifth day was, how-

ever, only one element of collapse. The brusque defer-
vescence was another,—at least as important ; and a
superficial reference to the fact, that digitalis has been fre-
quently employed with success as an anti-pyretic, might
lead at once to the conclusion that it had caused the fall
of temperature in this case.

Assuming for the moment that this were possible, the
cardiac depression (of force) could be explained either by
the sudden fall of temperature itself (from 103° to 97°, or
six degrees), or by the conditions causing that change. The
influence of digitalis, therefore, would depend on the nature
of its antipyretic action.

The following explanations have been offered :

Theory of Anti-pyretic Action of Digitalis.

That digitalis lowers febrile temperature by acting on the
vaso-motor system, causing relaxation of peripheric vessels
through whose contraction the elimination of heat has been
arrested (Traube).

That it contracts the peripheric vessels, thus causing
acceleration of peripheric circulation, and thus more exten-
sive cooling of the surface (Ackerman, Heidenhain).

That the antipyretic action depends on retard of circu-
lation and diminution of arterial tension (Nothnagel).

That digitalis directly diminishes movements of organic
combustion (Mégevand, quoted by Bernheim).

That digitalis, like all the medicines which lower the tem-
perature, and keep it lowered during some time, acts
directly upon the nervous centres which preside over calori-
fication, and changes the morbid modality in virtue of
which the organism regulates its heat at a high degree
(Bernheim).

The narrow limits of our purpose, which aims only at the
analysis of a single case, does not permit discussion of the
obscure and profound questions involved in any of the fore-

going hypotheses. It suffices to notice, as has already been done in many criticisms upon them, that an increased elimination of heat cannot suffice to arrest the febrile process, since, in fever, the elimination as well as the production of heat, is already greatly above the normal standard ; and that the antipyretic effect of digitalis cannot be explained by its action on the heart, because the first effect is not at all proportioned to the second.

Anti-pyretic Collapse.

On the other hand, any agent that really, and for a long time, depresses febrile temperature, *must* affect the increased organic combustions associated with the fever. The elimination of urea would not, however, be at once diminished, because that which had resulted from anterior retrograde metamorphoses of tissue would for some time be in excess of the norm, even when these metamorphoses had been arrested. Whether the antipyretic action be exerted directly on the molecular nutritive processes in the tissues, or indirectly upon them through the medium of the nerve-centres, it is certain that a profound perturbation must always be introduced into these processes. This, if carried beyond a certain limit, may so far reduce the nutritive exchanges of the tissues, that the vitality of the latter become temporarily impaired. Hence the collapse, of varying degree, which is always liable to attend every artificial, and even every spontaneous, defervescence.

Collapse depending on a generalized depression of molecular nutrition, involves the heart in two ways. The nutrition of the heart is perturbed, together with that of all other tissues, and the capillary circulation is retarded more and more, thus offering increasing obstacle to the work of the heart, until this is liable to be arrested by the peripheric resistance.

To these dangers must, in the case of digitalis, probably be added the danger of direct paralysis of the cardiac muscle, liable to be induced as a secondary effect, to its original functional stimulation by this drug, and especially with the excessive doses habitually required to depress the temperature.

In Mrs. A's case these doses were not used. The only possible way in which the digitalis could have been responsible for the collapse of the fifth day, would have been by accumulating in the organism. But forty-eight hours was certainly too short a time for such an accumulation to be effected. We must conclude, therefore, that the defervescence, with attendant collapse, must have been spontaneous. Such early collapse is always ominous, and often indicates an unusual extent of intestinal lesion. Hamerynk describes cases where *all* the glands of the intestine, from the duodenum to the rectum, were infiltrated, and death occurred with choleriform symptoms.—(Quoted by Griesinger.) In Mrs. A's case, diarrhœa did not set in till the ninth day, thus four days after the first attack of collapse. But a generalized infiltration of the small intestine may have begun much earlier. The second attack of collapse followed the first onset of diarrhœa ; and this fact makes it the more probable that the first originated in extensive irritation of the abdominal nerves from the typhous infiltration. In this case the irritation must have been reflected at once to the nerve-centres, overwhelming them and depressing the temperature ; and to the intra-cardiac nerves, weakening the force of the ventricular contractions,—hence of the pulmonary circulation, rendering imminent pulmonary hypostasis.

The long apyretic period which followed the first collapse, although the heart's action regained its force and the pulmonary congestion diminished, showed sufficiently that

the cardiac and nervous collapse were independent of each other.

And now the important practical question presented itself,—and even more urgently in reviewing the case than in watching it,—whether, during a defervescence that had occurred independently of the action of the digitalis, the usual rule, which requires suspension of the drug as soon as the temperature falls, should be observed. It was assumed that the small doses would act as a cardiac tonic ; and as the number of beats had not been depressed, there seemed no reason for discontinuing them. A further rule, laid down by Wunderlich, Hirtz, and Bernheim, is that if a very rapid pulse continues to resist the digitalis, the drug must be abandoned, as it is then liable to become dangerous. But Mrs. A's case did not seem to come under this rule, for the number of beats was not excessive, being only 100.

The point of possible danger lay in the possibility of slow accumulation of the drug in the system. To us it still remains uncertain, how far this danger was really incurred in Mrs. A's case. It is, therefore, also a question, and a grave one, how far the remarkable succession of attacks of collapse, which marked the second period of the fever, indicated a struggle of the heart with a remedial poison, in addition to the poison of the disease, or whether the heart was sinking in spite of the best aid which could be afforded it. It was, of course, the latter supposition which led to the continuance of the digitalis. On the affirmative of this question must be reckoned : 1st. The known tendency of the drug to accumulate ; 2d. The cyanosis which accompanied the sinking attacks, becoming more pronounced as these were more frequent. Cyanosis is a marked feature of digitalis collapse. It seems to depend on the accumulation of blood in the veins which results from the low arterial tension.

On the other hand, an unfavorable influence of digitalis seemed to be negatived by : 1st. The immediate increase in cardiac force that followed its administration on the night of the 10th, after an interruption of several hours ; 2d. The increase in the number and severity of the collapse attacks during the four days (from the 13th to the 17th) that the digitalis was suspended. A favorable influence seemed also to be inferred from the diminution of the pulmonary symptoms under its use. The pulmonary hypostasis at first threatened was averted, and although a limited pneumonia subsequently developed, it did not seem to determine the fatal issue. At all events this pneumonia was under the direct influence of the typhic poison, not of the heart failure.

In regard to the latter point, however, it is probable that the interruption was not sufficiently prolonged to really test its effect, since the effect of digitalis persists for more than four days after cessation of a dose that can be shown to have produced one. As I myself only saw the case at intervals, and only five or six times altogether, I cannot tell what impressions might have been derived from a more continuous observation. It is noticeable from the chart that on one occasion at least, the pulse fell to 66, and this certainly should have been a signal for interrupting the digitalis. But when, on the contrary, the medicine was continued, the pulse rose again.

A case treated with digitalis by Sydney Ringer, and published by Morell in the *Practitioner* for 1873, has an important bearing on the dangers of the possible accumulation of even moderate doses. The patient was a young girl, and no cardiac degeneration existed previous to the fever. The drug was administered continuously, at first in doses of one grain of the powder ; afterward in doses of nine, then twelve, minims of the tincture. The dose was increased

when the pulse became more feeble, and the medicine continued after the frequency had disappeared; an evident error. The patient died suddenly in syncope.

The above case is of very great importance for our purpose, since, except in the age of the patient, it bears a considerable resemblance to Mrs. A.'s.

Liebermeister, in *Ziemssen's Handbuch der Medicin*, and later in the *Handbuch der allgemeinen Therapie*, declares his dissatisfaction with digitalis as a remedy for the reduction of a rapid, weak pulse, with high temperature, in typhoid fever. He thinks it generally fails, and sometimes even *seems* to accelerate the cardiac paralysis.

Bartholow, in the last edition of his Treatise on Materia Medica and Therapeutics (1884), quotes rather summarily from Liebermeister, and, on the authority of the latter, throws over the use of digitalis completely in the emergency described. Murchison says he has no experience whatever with it.

The foregoing case has been analyzed in detail, because it is thought that in this weighty matter, when such important authority may be quoted on both sides, that a single detailed clinical observation is of value. If the observation does not positively demonstrate a dangerous influence exercised by digitalis,—it must at least be counted as a case in which this "cardiac tonic" altogether failed in the very emergency for which it has been so warmly recommended.

Taken together with the case of Sydney Ringer's, ours probably illustrates the truth of the dictum of Hirtz, that small doses of digitalis given for a long time are more dangerous than large doses given at the outset of the disease for a short time. It may be inferred, also, that if small doses are given, without apyretic intention, but as a cardiac tonic, it would be better to give them according to the method in use in chronic heart disease: namely, ten to

twenty drops of the tincture three times a day. Further, that in any case,—*i. e.*, whether the medicine seemed to be doing good or not,—it should be suspended every three or four days for a period of four or five days. A shorter period does not suffice to test the effect of the interruption, since the influence of digitalis on the pulse, though not on the temperature, remains perceptible during four of five days after the cessation of its administration.

Out of the fifty-three cases of typhoid fever treated with digitalis by Bernheim, eight proved fatal. In three cases death was due to an intestinal perforation; in three the digitalis was given *in extremis* in an advanced stage of the disease, when the heart was fatty and pulmonary hypostasis already established; in one case the patient was alcoholic, with fatty heart; the eighth case succumbed to intestinal hemorrhage.

"When the cardiac muscle is softened and the lungs engorged, it is too late for digitalis; this drug is even formally contra-indicated, because, if it acts at all, it will be to slacken the heart without reinforcing it; it may, therefore, precipitate a fatal collapse" (Bernheim, *loc. cit.*, p. 437).

"Nor," continues this writer, who, with his master, Hirtz, is one of the most enthusiastic advocates of digitalis, "would I affirm that whenever digitalis is given in time it will prevent the heart from degenerating. In typhoid fever we are never able to count *with certainty* upon the efficacy of digitalis: the antipyretic action, like that on the pulse, may entirely fail, or be only temporary. But the *most often* this influence is sufficiently durable to avert one of the greatest dangers of the disease, that which results from cardiac feebleness" (p. 438).

To this statement we must add: When that feebleness depends on the influence exercised on the cardiac nerves by the febrile process, and is unassociated with structural

alterations, either caused by the disease or anterior to it ; when, also, the cardiac weakness is associated with high temperature; when, as in Mrs. A's case, the temperature falls, digitalis, even in small doses, seems likely to be useless, if not injurious. It is possible that it prove injurious, as in the fatal cases of Ferber and Ringer (see above), when fall of the pulse was no more marked than in favorable cases, or when it did not occur at all, as in ours,—when, also, vomiting, a most characteristic sign of digitalis narcosis, is absent.

For this drug, as for chloral, morphine, and many others, prolonged resistance on the part of the organism to its characteristic effects should be a warning not to push, but to cease, the administration, lest cardiac collapse occur suddenly.

Apart from the influence of digitalis, the observation illustrates two facts in typhoid fever : 1st. The extreme difficulty of combating the symptom of heart failure, implying failure in the nutrition of the heart muscle, by either general or special stimulants ; 2d. That the danger of typhoid fever is by no means exclusively proportioned to the rise of temperature. Since, in the absence of high temperature, the cardiac depression and generalized nutritive collapse must be due to the febrile poison alone, and since this agent is constantly present, together with high temperature, when that exists, it is impossible to ascribe either the direct or indirect symptoms of heart failure exclusively to the temperature, even when this is high. We may repeat the question, which has already been asked, how far the value of artificial depression of temperature by antipyretic treatment, and especially by cold baths, does not consist in a temporary interruption to the multiplication of the infectious agent in the organism.

A CONTRIBUTION TO HERNIOLOGY.

By ARPAD G. GERSTER, M.D.,

SURGEON TO THE POLYCLINIC, AND TO THE GERMAN AND MOUNT SINAI HOSPITALS.

THE cases here presented possess sufficient importance to be brought to public notice. One illustrates a rather unusual cause of failure of a timely herniotomy ; the second, the occasional necessity for resorting to the most ancient form of radical operation of hernia, viz., that supplemented by castration ; the last one a normal operation of a rather complicated case followed by the radical operation, as practiced by German surgeons at the present day.

CASE 1.—Joseph Mandelbaum, a well-preserved man of seventy-three years, had had an oblique inguinal hernia on the right side for twenty years. The patient was always able to replace the tumor except on two occasions, when the aid of a physician was required to accomplish reduction. He had worn a truss since ten years. The tumor had come down thirty-eight hours before his admission to Mt. Sinai Hospital, on Aug. 21, 1883, and a physician had attempted taxis unsuccessfully. Very severe abdominal pain, nausea, with occasional vomiting, great excitement marked by volubility of speech, and the absence of alvine evacuations gave the case a rather grave aspect ; but the temperature being normal, and in the absence of tympanites, or signs of local inflammation about the tumor, it was decided to administer a generous dose of an anodyne, together with an ice-bag over the swelling.

During the subsequent seven hours the patient's thirst becoming unquenchable, and his restlessness excessive, it was decided to try operative relief. The tumor was four inches long, and about

three inches wide, not painful on touch, its tension moderate, and it gave resonance on percussion. Ether being administered, a gentle attempt at reduction was made, without success, whereupon the sac was exposed by a suitable incision extending well up to the external abdominal ring. By invaginating the tip of the finger into the inguinal canal, a few fibres connected with the internal pillar were felt to present an obstacle to reduction. These being divided, the contents of the sac could be readily slipped back into the abdominal cavity without opening the hernial sac. The wound was irrigated with a 1:2000 solution of bichloride of mercury, was closed by a few silver-wire sutures, and dressed with a strip of iodoformed muslin.

Six hours after the operation a temperature of 99.5° F., with 98 pulses, was recorded. Within six additional hours fever came on, with delirium and deep depression, the pulse not rising in proportion to the temperature. The former did never exceed 105 beats, and was mostly varying between 75 and 100 per minute ; the temperature, however, gradually rose to 104.4° F. and remained there until the patient's death, which took place thirty-nine hours after the operation. The wound showed no reaction throughout, and although the patient complained of pain in the belly, pressure did not increase it, and there was no meteorism whatever.

The post-mortem examination made four hours afterward revealed the following facts : Primary union of the external wound, which being forcibly opened, about a drachm of bloody serum was seen to collect in its bottom. The sac being incised, it was found to be empty, its serous lining pale and smooth, and through the inguinal canal the finger was passed into the abdomen, and the abdominal wall was divided. The peritoneum was found normal ; in the bottom of the pelvis were a few drachms of limpid serum. The intestines presented a normal, pale aspect, except about three feet of ileum, which appeared rosy, and slightly distended by gas. This being gradually raised out of the belly, a well-circumscribed, slate-colored and shrunken part of the gut came to view, extending over two and a half inches of the convexity. Corresponding to this necrosed portion of the ileum, an old and firm cicatricial cohesion of several coils of intestine was found to exist, undoubtedly due to a former inflammatory process. Here the mesentery was thickened and œdematous, showing purplish discoloration, and *a number of hard and much enlarged mesenteric glands* were found to form the nucleus of this dense cicatricial tissue. There was no

adhesion to the abdominal parietes. *Cause of death :* Septicæmia due to necrosis of ileum.

It seems probable that necrosis was produced more by compression of a terminal mesenteric artery of the gangrenous knuckle, through undue and prolonged traction, than by direct strangulation of the gut. This view is justified by the slight impediment requiring cutting, and that the cause of incarceration was found outside of the sac.

Furthermore, it is fair to assume, that necrosis of the gut had commenced before the operation, producing symptoms of pain and depression, but that decomposition and absorption of septic matter must have set in later than six hours after the operation, when their presence and that of n-cipient peritonitis were indicated by septic fever.

It is said that the extra-peritoneal operation, as illustrated by this case, is safer than that accompanied by opening the sac. In the light of the experience furnished by the preceding case, and possessing the aid of antiseptic precautions, we must concede that this view needs material correction. Had the sac been opened in this case, inspection may have revealed the true state of affairs; and excision of, or even a simple incision into, the decayed gut might have saved the patient's life.

CASE 2.—August Bentlage, æt. twenty-four, a rather slender metal-worker, whose pale skin was dotted with a number of pigmented moles, was admitted to the German Hospital, August 17, 1883, giving the following history :

He had a hernia since childhood, never wore a truss, and felt no inconvenience whatever until this summer, when he commenced to be troubled by frequent and very sharp griping pains in the belly, which subsided only when he assumed the recumbent posture. Being unable to do work, he begged to be relieved by an operation. On examination, a right oblique inguinal hernia was made out, consisting principally of omentum. It could be readily reduced to a certain point, the testicle following the hernia up to the external abdominal ring. When the testis was pulled down,

the finger meanwhile retaining the hernia, unbearable pain and nausea were complained of. On further examination, a band-like mass could be felt running from the anterior aspect of the testicle into the hernial mass. The patient never had tried to wear a truss, wherefore several attempts were made to fit him with one ; but all resulted in causing intense pain. The patient was visibly emaciating, urgently demanded relief, and, on Aug. 23d, a hitherto unnoticed knotty swelling was detected, occupying a certain part of the hernia. Malignant disease of the omentum being suspected, the following operation was performed at once : The testicle, together with the hernia and tuberous mass, were well brought down, and the sac was opened. It became plain that we had to deal with a congenital rupture, the omentum and the gut occupying the tunica vaginalis. The omentum was firmly attached to the anterior surface of the testicle, and presented, in its middle, the appearance of a nodular pigmented mass, surrounded by unchanged omental tissue. The inner surface of the sac itself and the testis, were also sprinkled with a large number of lenticular pigmented spots. The testis and cord were somewhat atrophied. First the cord was drawn out, severed, and the vessels tied with a number of catgut ligatures ; then the omentum was ligatured and cut off ; the neck of the sac was separated from the surrounding tissues, and was forcibly drawn out of the wide inguinal canal, till a place was reached about half an inch beyond the last pigmented spots. Here a purse-string-like suture of catgut was passed in and out alternately, through and around the circumference of the sac, and was firmly tied, the stumps of the cord and omentum having previously been returned into the abdominal cavity. After this the sac was cut off below the circular suture, and was dissected out of the scrotum, in one mass, with the testicle. The external wound was closed with a few stitches ; no drainage-tube was inserted.

No local or general disturbance followed the somewhat severe operation, the wound healed by adhesion, and three weeks after the operation the patient could be permitted to leave his bed, using a truss with comfort. Notable were afterward a pertinacious constipation and gastro-intestinal catarrh, requiring internal treatment till Nov. 26th, when he was discharged cured, and in excellent health.

Cut into, the nodular mass was found to be composed of a mottled, dense, sarcomatous tissue. The vas deferens

and vessels of the cord were rather slender, but the cremaster showed a remarkable state of hypertrophy, due, without doubt, to the constantly provoked reflex phenomenon of the testicle being tightly drawn up to the inguinal canal.

In this case rapid growth and local dissemination of a pigmented sarcoma of the omentum was caused by the pressure of a truss. The sarcoma had originated in cicatricial tissue, produced by a former local peritonitis.

CASE 3.—Josephine Reinhardt, fifty-four years old, married ; a well-nourished, hard-working woman ; has had, since many years, a large omental hernia of the left side, never wore a truss, and exhibited symptoms of incarceration since three days. On November 15, 1882, stercoraceous vomiting set in, and Dr. Guden requested me to see the patient. I found her pulseless, the skin covered with a clammy perspiration, the fetid vomiting almost constant. The integument over the hernia was much suggillated : the effect of repeated unsuccessful attempts at taxis. The case admitted no doubt, and the patient was at once brought under the influence of chloroform. During the struggle of the stage of excitation the hernia suddenly enlarged in size by about one third, the skin over it becoming tense and shining. A cutaneous incision of seven inches laid bare the sac, which being opened about half an ounce of bloody serum escaped. A mass of hypertrophic omental tissue, the size of a man's fist, became thus visible, which being deligated in seven portions with stout catgut, and cut away, a small knuckle of purplish-blue lustreless gut was exposed. The strangulation was found to be situated within the external inguinal ring, which on being incised, offered no resistance to the replacement of the omental stump. The gut was drawn out well for inspection, and its reflex contractility on pinching still being preserved, I deemed it advisable to replace it in spite of its bad appearance. The gut and the sac were well washed with strong carbolic solutions, then the former was slipped into the abdominal cavity. The sac was dissected out, up to its neck, and was cut off below a purse-string suture encompassing the neck of the sac and the external abdominal ring. After this the large external wound was united with a number of catgut sutures, a small drainage-tube being placed in the lower angle of the wound. An antisep-

tic dressing and spica covered the whole. A hypodermic morphine injection was given, and the pulseless patient was put to bed. The vomiting had entirely ceased, the pulse rallied, and a large quantity of flatus passed during the night. The temperature rose to 101.5° F. the next morning, but her general condition being subsequently satisfactory, the dressing remained undisturbed. Some trouble was experienced in establishing a free movement of the bowels, but this having been accomplished on the fifth day, she could be declared as convalescent. Catheterism, which was needed until then, also became unnecessary. The first dressing was changed November 20th, and with the exception of the opening left by the drainage-tube, the entire wound was found healed by first intention. The next change of dressings occurred November 28th, when a firm cicatrix was found in the whole extent of the incision. Five months later I found, on removing the truss, no hernia, and very little of impulse on coughing, but advised to wear the truss constantly.

This case may be accepted as a fair illustration of the modern radical operation as practised in Germany. It does not accomplish a radical cure in such a sense as to obviate the necessity of wearing a truss. Experience has shown that in these so-called cured cases a relapse is apt to occur on slight provocation, after the usual shrinking and softening process of the cicatrix has been well advanced. But its advantages are nevertheless so great that, whenever practicable, this slight addition to the usual herniotomy should not be omitted. After it the patient gets along very well with a light truss, which also is amply sufficient to prevent a recurrence of the rupture.

In most cases the dissection of the sac is very easy; I generally succeeded in stripping it out of the surrounding connective tissue without the use of a cutting instrument. It is proper to remark, however, that some cases are on record in which the dissection of the sac was reported to have been very troublesome.

MORPHIA AND GELSEMIUM.

By Isaac Ott, M.D.

A S is well known, some drugs when given together increase the action of each other, whilst others antagonize. Thus the preliminary use of morphia prolongs the effect of chloroform, whilst atropia antagonizes the activity of the first.

In this paper I shall study the effect of a previous dose of morphia on the toxic activity of gelsemium, whether it accelerates it or retards it.

Gelsemium for its poisonous virtues depends on two bodies, gelsemia and gelseminic acid, the latter being mainly a convulsivant. That gelsemium is a very powerful agent is attested by the number of cases of poisoning resulting from it, many of them fatal.

In one case of poisoning the subsequent use of morphia was thought to have acted beneficially. In this paper I shall try to show that the preliminary use of morphia increases the rapidity of the toxic action of gelsemium. The experiments were made with a fluid extract of gelsemium, and upon kittens either of the same litter or as nearly as possible of the same size. The weight of the animals is in pounds.

EXPT. 1.—Kitten received at 2.1 P.M. one half a grain of morphia subcutaneously. At 2.10 P.M., twelve minims of the fluid extract of gelsemium ; the animal is very lively, rotates in a circle ; posterior extremities weak. 2.28 P.M., twelve minims of fluid extract of

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gelsemium. 2.30 P.M., convulsive movements. 2.43 P.M., difficult respiration. 2.55 P.M., kitten dead ; when the death agony commenced it was rapid. Weight of animal, $1\frac{5}{60}$ pounds ; death in forty-five minutes.

EXPT. 2.—Kitten of same litter as the one used in expt. 1 ; received thirty-four minims of fluid extract of gelsemium. 2.15 P.M., lying down, trembling about the lips and head. 2.28 P.M., twelve minims of fluid extract of gelsemium. 2.31 P.M., trembling movements of head. 2.55 P.M., difficult respiration ; head falls between the anterior extremities. 3.12 P.M., death slow in its nature. Weight, $1\frac{9}{60}$.

EXPT. 3.—Kitten received subcutaneously one half a grain of morphia at 1.15 P.M. 3.25 P.M., limbs weak ; when he attempts to walk the limbs spread apart ; animal is dazed. 7.35 P.M., in dazed condition ; does not know what he is about ; recovered next day. Weight, $1\frac{9}{60}$ pounds.

EXPT. 4.—Kitten at 3.15 P.M. received one half a grain of sulphate of morphia. At 3.25 P.M., twenty-four minims of fluid extract of gelsemium. 3.32 P.M., circus movements. 3.35 P.M., lies on his sides, utters cries; inco-ordination ; pupils dilated ; claws with extended paws in order to move. 3.40 P.M., can raise his head up, rotates on his long axis. 4.5 P.M., cat is able to cry and struggle ; muscular tonus is good, although respiration is irregular ; jerking inspiration. 4.15 P.M., dying, although able to move his limbs. 4.16 P.M., death. Weight, $1\frac{48}{60}$.

EXPT. 5.—Kitten at 2.30 P.M. received twenty-four minims of fluid extract of gelsemium. 3.35 P.M., seems unconcerned, and sits upright as usual. 3.43 P.M., trembling about the muscles of the neck. 4.15 P.M., lies with head on the floor, as if asleep ; can walk very well ; muscular tonus lost ; muscles soft. 4.16 P.M., can run over the floor ; great difficulty in keeping the head elevated, much more so than in cases where morphia had previously been used ; muscles greatly relaxed ; cannot struggle violently, or raise himself upon his feet. Death at 4.43 P.M. Weight, $1\frac{43}{60}$.

EXPTS. 6, 7, 8.—Three kittens received at 1.30 P.M. half a grain of morphia. At 1.45 P.M. they received twelve minims of the fluid extract of gelsemium subcutaneously. At 2.10 P.M. all were profoundly affected ; inspiration quick, difficult ; expiration easy.

Death of 1st at 2.32 P.M. Weight, $1\frac{47}{60}$ pounds.
" " 2d " 2.34 " " $1\frac{44}{60}$ "
" " 3d " 2.43 " " 2 "

EXPTS. 9, 10.—Two kittens received at 2.45 P.M. twelve minims

of fluid extract of gelsemium subcutaneously. 3 P.M., walking about ; drink some milk. First died at 4.9 P.M. Weight, $1\frac{2}{50}$. The second cat recovered, although profoundly affected by gelsemium. On the following day the same cat, at 1.15 P.M., received a half grain of morphia, and immediately after it, twelve minims of the fluid extract of gelsemium. Death at 2.10 P.M. Weight, $1\frac{46}{50}$ pounds.

In examining the above experiments, the results will be found as follows :

Expt. 4, morphia, $\frac{1}{2}$ gr.; gelsem., 24 minims; weight, $1\frac{48}{50}$; death in 51 minutes.
  "   5   "   24   "   " $1\frac{43}{50}$ "   " 73   "
  "   1, morphia, $\frac{1}{2}$ gr.;   "   24   "   " $1\frac{15}{50}$ "   " 45   "
  "   2   "   46   "   " $1\frac{9}{50}$ "   " 63   "
  "   3, morphia, $\frac{1}{2}$ gr.;   did not die.
  "   6   " $\frac{1}{4}$ "   gelsem., 12 minims; weight, $1\frac{47}{50}$; death in 62 minutes.
  "   7   " $\frac{1}{4}$ "   "   12   "   " $1\frac{44}{50}$ "   " 64   "
  "   8   " $\frac{1}{2}$ "   "   12   "   "   2   "   " 73   "
  "   9   "   12   "   " $1\frac{29}{50}$ "   " 75   "
  "   10, morphia, $\frac{1}{2}$ gr.;   "   12   "   " $1\frac{46}{50}$ "   " 45   "

By an analysis of the above experiments, it will be seen that morphia and gelsemium combined kill quicker than gelsemium alone.

A priori it would be expected that two drugs like morphia and gelsemium, both paralysants of the respiratory centres, would accelerate each other's toxic action.

TWO CASES OF EMPYEMA SUCCESSFULLY TREATED BY "THRO' DRAINAGE."

By JOHN J. CONWAY, M.D., AND WILLIAM M. THALLON, M.D.,

BROOKLYN, N. Y.

CASE 1.—John P. H. Age, twenty-one. Born in Sweden. Shoemaker. Was sent into the Brooklyn City Hospital during our joint service on February 11, 1881, by Dr. Robert Ormiston, the Visiting Physician. Patient on admission was much emaciated, and so weak that he could not walk unaided. He had raised blood several times ; had considerable cough and profuse expectoration and frequent night-sweats. On physical examination (by Drs. Ormiston and Conway), the apices of both lungs gave all the evidences of tubercular deposit.

Diagnosis.—Pulmonary tuberculosis.

A generous nutritive stimulating diet was directed. The medication consisted in ol. morrhuæ and tinct. ferri chlor. three times a day, and extr. ergot. fl. according to indications. For the night-sweats he was given tinct. belladonnæ, ℳ vij, with good results.

Patient improved rapidly, gaining flesh, cough and expectoration ceasing, and on the advent of fine weather he was discharged on April 3, 1881.

He was readmitted to the hospital two weeks later (on April 17th) in almost as bad a condition as when he first came there. For the first few days no particular attention was paid to him beyond putting him on his former treatment, it being supposed that he was simply suffering from exacerbation of his old trouble. But having had several rigors, without a corresponding elevation of temperature to indicate an acute febrile condition, he was examined by Dr. Conway, who found the percussion note on the left side of his chest, as high as the third intercostal space, absolutely flat, and above this marked dulness. Pulmonary resonance on

this side of the chest could not be detected. The intercostal spaces were bulging and prominent; respiration rapid, and confined to the right side; skin cyanosed, and the patient's expression anxious and distressed. A diagnosis of empyema was made, and confirmed by the introduction of a hypodermic needle and the drawing off of a syringeful of pus.

General treatment having been tried without any benefit, it was evident that in an operation was the only source of hope. He was rapidly failing from day to day; pulse very feeble and rapid, 130 per minute. Respirations increased as high as thirty-six, and he was fast losing flesh. Temperature remained low, often subnormal, dropping a degree and a half (97°), the highest elevation in the week preceding the operation being 100½°.

With the kind consent of Dr. Ormiston, under whose charge the patient was, and after some consultation by letter with Prof. Rushmore as to the method of operating, the following operation was done on May 1, 1881 : The lowest point from which pus could be drawn with a hypodermic syringe was found to be between the tenth and eleventh ribs, on a line drawn vertically from the posterior fold of the axilla. Through this intercostal space a large trocar and canula were thrust; the trocar was withdrawn, and as the pus was gradually allowed to flow out, a long probe (a uterine sound served the purpose) was carried through the canula and passed, hugging the chest wall, upward and forward to the fourth and fifth ribs, about two inches to the left of the sternum. The apex beat of the heart was displaced about an inch and a half to the right of this point. The point of the probe being felt through the intercostal wall, was cut down on, and it was then thrust through this incision. The end of the probe having been bent on itself, a good-sized rubber drainage-tube, perforated, was fastened to it, and the whole withdrawn through the posterior opening. The amount of pus immediately evacuated was about fifty-five ounces, and there must have been about twenty ounces more on the dressings next morning. The ends of the drainage-tube were tied together, and the whole retained in place by adhesive straps. Thick wads of oakum, soaked in carbolized oil, were placed on the wounds, and a body bandage applied. The operation was done without an anæsthetic; ether was tried at first, but produced such alarming symptoms of failure of respiration and heart action, that it had to be discarded.

The next day the pleural cavity was washed out with five-percent. solution of carbolic acid, but this was soon diminished to

1 in 250. The drainage-tube was twice changed, smaller sizes being used, before it was finally removed. Suppuration diminished day by day, and when the carbolized water came through quite clear, the tube was removed on June 20, 1881.

At first, washing out was done twice a day, but later only once. Patient's temperature never reached 100° except on the eighth day, when it touched 100½°; respiration at once fell below 30 and gradually became normal; pulse similarly fell to below 100 and gradually diminished to normal. His improvement after the operation was immediate; he rapidly regained strength and appetite, and his weight increased upward of twenty pounds in two months. His chest condition when discharged on July 31, 1881, was as follows: Right lung normal; heart position normal, apparently; apex beat about a half inch to the inside of the anterior cicatrix and slightly lower. Left lung good; respiratory murmur: throughout, resonant on percussion, with some dulness below the level of the fifth rib, probably due to pleuritic thickening. The evidences of apiceal deposit had disappeared.

When last seen, in March, 1883, the patient stated to Dr. Thallon that he had worked at his trade steadily since his discharge from the hospital, and that he had not had any further trouble with his lungs or chest.

Case 2.—Johnnie M. Age ten. Born in U. S. Was accidentally shot on May 23d, 1883, with a pistol in the hands of a playmate. The pistol was loaded with a slug of lead. He was removed by ambulance to the Homœopathic Hospital, where he remained for eleven days. Owing to some dissatisfaction he was then taken home by his father, and Dr. Conway was called in on June 7th, fourteen days after the injury. He was hot and feverish and had a temperature of 103½°; pulse very rapid, and respirations over thirty a minute. Considerable cough and pain in the chest. The point of entrance of the slug could be distinctly seen—it was an inch and a half to the right of the sternum, between the second and third ribs. No point of exit could be discovered, and as no attempt had been made to extract the ball, it is reasonable to suppose it was lodged in the right lung. What made this more than probable was, that at the point of entrance through the cicatrix, one could feel distinct crepitation, and for some distance around the tissues were emphysematous. The whole right pleural cavity below the point of injury was filled with fluid, and the diagnosis was therefore made of traumatic pyo-pneumo-thorax.

The patient's hygienic surroundings were of the worst ; he lived in a damp basement, his food none of the best, and his parents ignorant people. The boy's general appearance was markedly tubercular, he had the square-shaped head, the clubbed nails, and the general appearance of that type. He began to rapidly lose flesh and the constitutional symptoms of blood-poisoning, rigors, high fever, sweats, foul tongue, etc., etc., became daily more pronounced. Having tried every thing he could think of in the way of general treatment—without any avail,—Dr. Conway suggested an operation as the only hope of saving the boy's life. To this the parents would not consent, and the patient continued to grow worse. Finally they realized how foolish they had been and urged the doctor to operate, which he reluctantly consented to do (regarding the case as hopeless) on June 21, 1883. We did exactly the same operation as in case 1. There were present : Dr. T. Mortimer Lloyd, who kindly gave chloroform, and Drs. B. F. Westbrook, A. R. Paine, Eddy, etc., etc. The lowest point posteriorly at which pus could be obtained was between the ninth and tenth ribs, about an inch external to the angle of the ninth rib. Instead of plunging in a trocar and canula, a scalpel was used but it did not prove quite so convenient. The long probe was introduced and carried up to and through the point where the ball had entered the chest-wall. A drainage-tube was then introduced and retained by the same manœuvres as in case 1. The amount of pus evacuated at the operation was seventy-two ounces ; the pleural cavity was immediately flushed out with a weak solution of carbolic acid ; large pads of marine lint were placed over the wounds and kept in place by a snug-fitting roller bandage. Hypodermics of brandy were freely administered and the patient put back to bed. It was the general impression of the gentlemen present that the patient would not have lived another twenty-four hours. He reacted well from the operation, and improvement at once began. The temperature record was not so uneventful as in the first case, for on the fifth day the thermometer marked 103° ; occasionally during convalescence there were such exacerbations of temperature, and they did not seem due to any retained pus in the pleural cavity but to some localized suppuration in the connective cellular tissue of the chest-wall along the inferior track of the drainage-tube. At first the pleural cavity was washed out every day, then every second day, and finally every third day. The drainage-tube was gradually withdrawn and finally removed on July 7, 1883, sixteen days after the operation. Pulmonary

resonance was then good over the whole right side; the cough and constitutional symptoms gradually disappeared. Crepitation at the point of entrance of the ball could no longer be felt or heard, the slug having probably become encysted. As there was considerable retraction of the right chest-wall and compensatory curving of the vertebral column, the boy was instructed to take forced and deep inspirations, and taught appropriate gymnastic exercises to correct the deformity; this he quickly and satisfactorily did. He was kept on quinine, iron, and digtalis, and the best food obtainable. He rapidly gained in flesh, and as soon as the drainage-tube was removed and the wounds firmly healed, he was sent to the Children's Seaside Home at Coney Island. His restoration to health was there completed, and he is now, six months after the operation, well and strong.

Remarks.—These two operations illustrate very well the success that may be attained in treating these cases under apparently the most unfavorable circumstances if the principal indication—of securing *complete drainage*—is met. It is because aspiration, or the employment of a single opening with a long compressible rubber tube, fails to meet this cardinal indication that they so often prove almost useless.

Another point of great interest suggested by the first case is that the moment the strength of the carbolic-acid solution used in washing out the pleural cavity was markedly decreased, the suppuration correspondingly diminished. This would seem to indicate that strong solutions injected into the pleural cavity tend to keep up the secretion of pus, and that if merely left to itself, or washed out by a solution whose cleansing action is mainly mechanical, the pleura is quite competent to take care of itself.

In other words, the irritation having stopped, the morbid secretion will soon cease.

In this connection it certainly seems difficult to justify the practice of some surgeons who inject caustic solutions of carbolic acid, tincture of iodine, etc., etc., to destroy the pyogenic membrane (?).

We have never heard of any surgeon who proposed to wash out the peritoneal cavity with caustic solutions after a traumatism or the removal of an ovarian tumor, even though purulent and septic accumulations have subsequently occurred. And yet, why would it not be as rational—or rather as irrational—as the similar proposal so often made in the case of empyema?

Both of these cases illustrate very well the extraordinary reparative power of nature and its tendency to resist pathological processes, and to cope successfully with a most formidable constitutional disease, provided the continuously irritating local cause is removed. That is all the surgeon should try to do, but it should be done thoroughly; and if it is done thoroughly, nature will do the rest.

EDITORIAL DEPARTMENT.

THE PROGRESS OF THE NON-RESTRAINT SYSTEM.

THE "non-restraint" system of treatment of insanity has now for four years been successfully carried out by me at the Kings County Insane Asylum.

On July 1, 1880, my first paper on the subject was read before the meeting of the National Conference of Charities, held at Cleveland, Ohio, "non-restraint" having then been carried out by me for six months preceding that conference. In that paper, subsequently published in the "Transactions of the National Conference of Charities," I pointed out the practicability of its adoption, the difficulties to be met with and overcome, and the advantages to the patients from such a method of treatment; how patients formerly troublesome, pugnacious, and destructive became comparatively quiet, docile, and industrious. I also pointed out the advantages of *occupation* for these insane patients, and *how it was almost a necessary part of the non-restraint system.* In an editorial in these ARCHIVES in February, 1881, at the completion of our first year of "non-restraint," I again pointed out the success we had met with and our entire satisfaction with the method. During this first year much opposition and adverse and unfair criticism, were offered by a large number of superintendents. Some indulged in sarcasm, and others even went so far as to say that we did not carry out non-restraint.

In April, 1882, another editorial in these ARCHIVES appeared giving our second year's experience with non-restraint, and also meeting a statement which had been quite generally made and

quoted by superintendents, to the effect that if we had abandoned restraint apparatus we used a large quantity of drugs, and "chemical restraint" was substituted for mechanical restraint in all "non-restraint" asylums. In that paper there were reproduced some tables drawn up by the late Dr. Wilbur (ARCHIVES, vol. vi., p. 271) from statistics of asylums in Great Britain, Canada, and the United States, and these tables fully showed that the allegation was unjust and unfounded; but, on the contrary, with non-restraint less sedative drugs were used.

It is now the closing of the fourth year of the non-restraint system in the Kings County Insane Asylum—a length of time which fully justifies the assertion that it is now clearly established in this country that non-restraint is practicable, and is a beneficial method of treatment. It is now so thoroughly a part of the organization of the Kings County Asylum, that one would hardly know that restraint apparatus had ever been used there.

It is unnecessary at this time to again speak of the advantages and method of carrying it out, but we will inquire what influence its adoption and success at the Kings County Asylum has had on the management of other asylums in America. Has it influenced other asylums to favor and adopt this method?

This can best be shown by quoting briefly from recent reports of some of the superintendents.

Dr. P. Bryce, Medical Superintendent of the State Insane Hospital at Tuscaloosa, Alabama, in his report ending September 30, 1882, says on this subject:

" In no department of sanitary science or social science has more progress been made than in the treatment and care of the insane, and it has been our aim to keep as well up as possible with this progress in the general management of our institutions.

" In the treatment of our patients we have *almost entirely succeeded in discarding mechanical restraint* of every kind; and our intercourse with them is characterized by the utmost kindness, candor, and courtesy. There are no leather mittens, muffs, bed-straps, restraining chairs, shower baths, or other terrifying apparatus in use in this hospital.

"In passing through our hospital, especially among the most excitable class of insane, visitors are surprised to witness so much order, quiet, and contentment on every side. They find the inmates well dressed, courteous, and often cheerful in their demeanor.

"We believe it quite possible that this principle of non-restraint may be carried so far as to be injurious in individual cases. We have often refused to allow a patient to be restrained when we knew that a little coercion would do him good.

"The great objection to the use of mechanical apparatus for coercing a refractory patient is its liability to be abused. It is easier, safer, and far better, on the whole, whatever may be the advantages lost in a few individual cases, to discard it entirely when it can be done."

Dr. A. B. Richardson, of the Athens Asylum (Ohio), for the Insane, in his report for 1882, in a number of published tables recorded no instance of restraint, and he says: "The amount of mechanical restraint used during the year has been almost absolutely nothing."

In the State Hospital for the Insane at Norristown, Penn., in his report of 1882, Dr. Robert Chase, the superintendent of the department for men, says: "I am convinced that employment and non-restraint are twin sisters that must go hand in hand in the upward march of improvement in the treatment of the insane."

In the report of Dr. Alice Bennett, the superintendent of the department for women, she says: "No mechanical restraint has been made use of during the year." Dr. Bennett is a strong advocate of non-restraint, as shown by several published articles.

Dr. Dewey, the Medical Superintendent of the Eastern Hospital for the Insane at Kankakee, Illinois, in his report for 1882, says: "The restraint on the male side has been very much less than in the last biennial period; with nearly double the number of patients there were only nineteen instances of restraint for a total of one hundred and twenty-five hours. The restraint during the last year of the period was much lessened (only six occasions); and during eleven months only one instance, with a

daily average of over two hundred patients. I have also to report that during the last five months there has been no case of restraint."

Dr. Goldsmith, the Medical Superintendent of the State Asylum at Danvers, Mass., in his report for 1882, says that the use of restraint apparatus in that asylum is a little less than half of one per cent. of the average population.

In the examinations of the Senate Committee of this State, appointed to investigate the asylums, in their session held in New York in the autumn of 1880, Dr. Carlos McDonald, of the Asylum for Insane Criminals at Auburn, N. Y., who then argued in favor of restraint, is, in 1884, carrying out the non-restraint system in his asylum. In his report for 1882, in a foot-note, he says: " No form of mechanical restraint has been used since March, 1882," and in his report for 1883, gives considerable space to its consideration and the results with this method in his management.

The above quotations from the reports of superintendents in various parts of the United States, who are all well known and conducting large institutions of their respective States, show clearly the progress which has been made in this direction by the thoughtful and progressive superintendents.

To learn more fully what progress had been made in this State (N. Y.) I appealed to the State Commissioner in Lunacy, who has kindly furnished me with some information taken from his last report to the Legislature (not yet published). He says:

" During the year the subject of restraint has been specially studied in all of the institutions for the insane. In the poorhouses in which the disturbed insane were found, restraint, in all of its forms hitherto known, is the ordinary method of control. This treatment of the disturbed class in poorhouses seems to be inevitable, owing to the want of facilities for isolating them under conditions favorable to their care, and of proper attendants. In the small county asylums there has been a marked diminution in the amount of restraint, and in several it has been discontinued altogether. This favorable change, it is noticed, has occurred

simultaneously with improvements in the accommodations for the insane, and in the employment of more and better attendants. In the larger county asylums less restraint is recorded than formerly ; the kinds employed are comparatively mild, and, as at Ward's Island Asylum, it is placed under rigid supervision. The King's County Asylum is the only one which employs no restraint whatever. In the State asylums the amount of restraint is diminishing, and in the State Asylum for Insane Criminals has not been employed in two years. Even in Willard Asylum, with a population of 1,750 chronic insane, there have been days when but one person was restrained, and in that case the simplest and mildest form was used."

As this article has already exceeded its intended limits, we will conclude by answering the question asked earlier in this paper : What influence its adoption (non-restraint) and success at the Kings County Asylum has had on the management of other asylums in America. Has it influenced other asylums to favor and adopt this method ?

The above extracts and information give us the data for answering this question as follows :

It has markedly influenced the management of all the asylums in the United States ; it has induced many to try and adopt the method, and in a great many to very decidedly diminish the amount of restraint formerly used. It has been shown that a method of management, which previous to 1880 was considered impracticable, is really easy of adoption, and will some day not far distant become almost universal in this country.

The foregoing is the history of the introduction of the "nonrestraint" system of treatment of insanity in this country. It shows that its success at the Kings County Insane Asylum, where it was first used, has been the inspiration and support of its use in similar institutions in the United States. The opposition and ridicule with which its introduction was greeted have happily given place to a spirit, first of partial trial, and then of cordial adoption, by those who witnessed or accepted the testimony of its feasibility and success.   JOHN C. SHAW.

NEW BOOKS AND INSTRUMENTS.

A Treatise on Bright's Disease of the Kidneys: Its Pathology, Diagnosis, and Treatment, with Chapters on the Anatomy of the Kidney, Albuminuria, and the Urinary Secretion. By Henry B. Millard, M.D., A.M. New York : William Wood & Co., 1884, pp. 246.

In the multiplicity of books of the present day the reviewer in taking up a new volume feels like apostrophizing it somewhat after this fashion :

" For what purpose are you written ? Why do you claim the attention of the reading public ? What have you that is worth adding to the common stock of ideas that you should come from a press already groaning with unprecedented fecundity ? Are you in existence because your author feels :

> " ' 'Tis pleasant, sure, to see one's name in print.
> A book 's a book although there 's nothing in 't ' ?

Is this why you come to an editor's table for favorable notice, fresh in your new covers and bright gilt-lettered title ?"

If it is a medical work that the reviewer has in hand so much the more sternly does he put his questions, and so much the more clearly must the book prove from its pages that it has valuable facts and discoveries to add to the already existing knowledge— facts and discoveries which, but for its author, would not be known.

Turning to the pages of Dr. Millard's treatise on Bright's disease with these thoughts in mind, we place before the reader that which the writer, beyond the management and compilation of his material, can claim as strictly original.

For his histology he depends mostly on Heitzman. From his

own investigations he claims that Heidenhain's rod-like structures of the epithelia which form part of a reticulum, considered by some the living matter proper, and intimately concerned in the process of secretion, give rise in the inflammatory process to a new formation of medullary and pus corpuscles. The flat endothelia, which have been recognized by recent observers as lying between the structureless (hyaline) membrane and the basis of the epithelia, are considerably enlarged in both catarrhal and croupous nephritis ; that they line the uninary tubules after the epithelia have been shed or lost, and that in croupous nephritis they surround the cast after the epithelia have perished in the formation of the cast.

After the introductory chapters on anatomy and histology of the kidneys, on albuminuria, including a discussion of its microscopic and chemical tests, the author passes on to deal with his subject, Bright's disease, which he considers under the three heads of croupous, interstitial, and suppurative nephritis.

Here the reviewer finds little in his diligent search for new ideas. In the etiology of acute croupous nephritis Dr. Millard illustrates by a case the fact which he thinks has not hitherto been mentioned, that essence of ginger will cause renal inflammation. He also brings forward two cases to support the view that malarial poisoning, though an infrequent factor, may produce acute nephritis.

The treatment is, after all, of the most importance to the practitioner, and the part of the book to which he will first turn. He will find the list of well-known remedies and abstracts of papers in the journals relating to the more recent ones which have been advocated, such as nitro-glycerine, helonias dioica, apis nullifica, euönymus atropurpureus. The author himself finds the chlorides of mercury the most efficacious. He uses the mild chloride in interstitial nephritis, and the corrosive sublimate in croupous. Of the mild chloride he combines one part with ninety-nine parts of the sugar of milk, which he gives in five- to ten-grain doses every two or three hours. Of the corrosive sublimate he prescribes $\frac{1}{10000}$ of a grain every two or three hours. Besides this, he administers three to six drops of a ten-per-cent. mixture of nitric acid three times a day.

The writer is chary of the metric system. He does not neglect it entirely, for he indulges in millimetres now and then ; but for the most part he expresses his dimensions in inches and fractions of inches and never deals in grammes or centigrammes. [G. P.]

Medical Diagnosis. A Manual of Clinical Methods. By J. GRAHAM BROWN, M.D., Fellow of the Royal College of Physicians of Edinburgh, etc. Second edition. Edinburgh : Bell & Bradfute. London : Simpkin, Marshall, & Co., 1883, pp. 370.

It is doubtful if more information on the subject of medical diagnosis could be crowded into a similar number of pages. All the means now employed for this purpose are briefly and concisely described, including inspection, palpation, auscultation, microscopical and chemical tests, the use of the sphygmograph, cardiograph, hæmacytometer, laryngoscope, and ophthalmoscope. It is not the book itself, but the tendency of such books, that one would criticise, which is to encourage a superficial knowledge of methods which should be more widely and deeply studied. Its value is similar to that of a small dictionary—useful for reference when the unabridged dictionary or the encyclopædia cannot be had.

[G. P.]

De la Démence Mélancholique. Par le Dr. A. MAIRET, Professeur agrégé de la Faculté de Médecine de Montpellier. Paris : G. Masson, 1883.

The author attempts the separation of certain abnormal cases of general paralysis from the general category, and their erection into a separate class. He justifies this attempt by a critical study of symptoms presented by his cases and a few from other authors, by their pathological anatomy, and by some experiments on dogs.

The semeiology of dementia melancholica may be summed up as follows : (1) a delirium of melancholic type, with ideas of persecution, of hypochondriasis and suicide, illusions and hallucinations—more especially of hearing,—often agitation and stupor; (2) early dementia, as evidenced by silly delusions, apathy, weakness of memory ; (3) certain physical symptoms, loss of muscular tonus of facial muscles, slight paresis of face or of upper lid, a muddy complexion with venous stasis near malar region, epileptic and apoplectiform seizures. Speech is often a little thick, but not vibratory as in general paralysis, and inequality of the pupils occurs ; no exalted delusions appear, and the melancholic delirium is not systematized. Post-mortem examination reveals thickening of the pia and adhesions to cortex (decortication), exactly as in general paralysis, but limited to the base of the brain, affecting more particularly the orbital gyri, the ventral portion of the fissures of Sylvius, and the temporal lobes in their medio-frontal aspects (gyrus hippocampi especially).

Dr. Mairet considers that the peculiar grouping of symptoms is due not to the nature of the lesion but to its localization at the base. He produced leptomeningitis and superficial cerebritis of the corresponding region in dogs by irritating injections, and noted in them a melancholic, stupid state, independent of any paralysis. In this connection the author does not, it seems to us, lay enough stress upon the coincidence of aural hallucinations, so frequent and strong in his cases, and the lesion of the temporal apex. These facts are strongly supported by Munk's experiments on the auditory cortical areas.

From a study of dementia melancholica, and from his experiments, the author would advance a new cerebral localization—that of depressed, hypochondriacal, and suicidal ideas in the basal and temporal gyri ;—a bold suggestion.

In dementia melancholica there is only a slight liability to extension of the lesion to the convexity, and the consequent change of symptoms to those of ordinary general paralysis ; in most cases the lesion remains localized at the base.

The prognosis and therapy are as unsatisfactory in this as in other varieties of organic dementia.

[E. C. S.]

Transactions of the American Gynecological Society.
Vol. 7, 1882, pp. 485. Philadelphia : H. C. Lea's Son & Co.

This volume is a very handsome memorial of the seventh annual meeting held by the Society in Boston on September 20, 21, 22, 1881. Like its predecessors, it is an excellent piece of book-making in all that concerns the publishers—type, paper, margins, and binding,—and in this respect it offers a pleasing contrast to the bad and dear books American medical publishers are wont to palm off on a docile profession. But the same praise cannot be awarded the editors. No explanation is offered for the great tardiness of publication, and, considering the fact that all the papers, by a by-law of the Society, are required to be handed to the Secretary within two weeks of the time of meeting, and the stenographic reports of the discussions, which were promptly sent to the individual members, had to be returned inside of ten days, there is great remissness somewhere.

Many of the monographs have been privately circulated for months, and the interest of others largely forestalled by other publications in the long interim between the time of meeting and the appearance of these proceedings. Similarly, the admirable

Index of Gynecological Literature is only for 1881, whereas the Index Medicus, from which it is transcribed, has just completed 1883. Lastly, certain of the papers are printed in a different order from that in which they were read, and though a small matter, as no note of explanation is offered, we call attention to it to condemn it.

Turning to the scientific papers, these number fifteen, including those by the two new Fellows, Dr. M. D. Mann, of Buffalo, and Dr. W. H. Baker, of Boston.

The first paper is the address of the President, Dr. Thomas Addis Emmet, of New York.

I.—Dr. Emmet departed from the usual presidential custom in presenting a strictly scientific paper on " A New Method of Exploration, with the Pathology and Treatment of Certain Lesions of the Female Urethra." The new method consists in the following operation, to be done under ether :

The patient being in the Sims position, a specially devised pair of scissors, like those used by seamstresses in cutting button-holes, is used ; the rounded blade in the urethra, the cutting one in the vagina, and closed in the median line, making an incision extending from a quarter of an inch from the urethral outlet nearly to the neck of the bladder. This may also be done with ordinary scissors and tenacula, a grooved staff made of block-tin being introduced into the urethra to keep the parts on the stretch. Should this operation have been done simply for purposes of diagnosis, the opening thus made can be closed by interrupted silver sutures, properly introduced, and allowed to heal. This will necessitate the patient remaining in bed for a week or more. If the operation is preliminary to further treatment, the urethral and vaginal mucous membranes must be stitched together and an urethro-vaginal fistula formed. On the completion of treatment, a second operation under ether will be required to close this artificial opening, much in the same way as is done for closure of a vesico-vaginal fistula. Dr. Emmet claims for this procedure that it is easy of performance, that it is superior to all other methods for purposes alike of diagnosis and treatment of all diseases of the female urethra. He narrates cases of new growths, urethro-cele, prolapse of the mucous and submucous tissues, laceration of the urethra from over-dilatation and from child-bearing, which he has cured by this button-hole method. He further states it as his opinion that it offers the best means of diagnosticating fissures of the neck of the bladder and treating urethritis. In the discus-

sion, Dr. Skene agreed that in the treatment of new growths and urethrocele this method was likely to prove of value, but claimed that it was inferior to the endoscope for purposes of diagnosis.

It is too soon to say just how much value this procedure has, until we have the experience recorded by other surgeons who have tried it, but it may be safely affirmed that it is by no means so easy or so safe as its originator would have us believe. The manner in which it is brought forward as a surgical panacea for all the diseases of the female urethra, cannot but militate against its general acceptance by the profession. Just as the more widely known and more valuable operation for restoration of a lacerated cervix, which we owe to the genius of Dr. Emmet, is in a certain disrepute, notably in England, because of the wild claims that have been made for it as a cure-all for uterine disease.

II.—" The Proper Use of Ergot in Obstetrics," by Dr. Joseph Taber Johnson, calls attention to the dangers attendant on the use of this drug, and the evils both to mother and child which result from its present indiscriminate employment. The conclusions the author reaches are :

1. It should *never* be given to a primipara.

2. It should not be given until after labor is completed, or if given prior to that, only when such dilatation is present as to permit of immediate instrumental delivery.

3. In placenta prævia, hemorrhage, and abortion, it should not be given until the uterine contents have been expelled or removed.

The one cardinal indication for its use he finds in atony and relaxation of the muscular walls of the empty uterus, leading to hemorrhage. The author bases these conclusions on the facts : that ergot produces a tonic and unyielding contraction of the muscular tissue of the uterus ; and that this contraction may begin at the cervix, and so imprison instead of expel the contents of the cavity, leading to possible death of the child and rupture of the uterus. In the discussion the members concurred in the main with these views, which are so much at variance with much of the teaching of the schools and text-books that they deserve widespread attention. A striking omission, seems to us, in the fact that no one called attention to the question of *dosage*.

III.—" The Ovarian Cell, its Origin and Characteristics," by Dr. Thomas M. Drysdale, is a reply to a paper on the same subject by Dr. Garrigues, read at the previous meeting. The author certainly succeeds in demolishing his antagonist's position pretty

thoroughly, but that his successs is equally great in establishing the scientific basis of his own, we are not competent to say. The sympathy of all right-feeling men will be with Dr. Drysdale in the disreputable trick which he was made the victim of by another member of the Society.

IV.—"Some Remarks on Ovariotomy with Special Reference to the Treatment of the Pedicle," by Dr. R. Stansbury Sutton.

This paper contains an historical sketch of the subject, together with the personal observations of the author of the practice of eminent ovariotomists in Europe. Much of the interest of this paper is forestalled by the chapter on ovariotomy in the recent edition of Lawson Tait's book.

V.—"Leucorrhœa Considered in Relation to its Constitutional Causes and Treatment," by Dr. Fordyce Barker. This paper calls attention to a most important and much neglected department of the subject.

The author quotes cases in support of his well-known views, that often, when local treatment has proved unavailing, the patient may be cured by appropriate constitutional regimen and improved morale. He justly urges that the symptom, leucorrhœa, may be the local expression of a constitutional condition; that it may be brought on by new modes of life involving increased nerve wear and tear; and that some of the local pathological conditions which receive such unremitting attention at the hands of enthusiastic gynecologists, are often the effects, and not the cause, of the leucorrhœa.

In conclusion, he points out that this side of the question has been unduly neglected, since the impetus given to local methods of treatment by the brilliant discoveries of Marion Sims.

VI.—"The Care of the Perineum," by Dr. Theophilus Parvin, is a short paper advocating support of that structure during labor, and, if need be, the employment of lateral incisions, to prevent too extensive laceration. The doctor quotes largely in support of his views, and is evidently somewhat burdened by his weight of learning.

VII.—"The Relative Value of Hysterectomy, and of the Complete Removal of the Uterine Appendages for the Cure of Uterine Fibroids," by J. Knowsley Thornton, M.B., C.M. (London). This article gives the statistics—especially of the London surgeons— of these two operations. The author's conclusion, that the latter is much the safer procedure, is corroborated by the recent work of Lawson Tait; but a much larger series of cases will be required

before any numerical expression of this result can be arrived at. In the discussion, Dr. Thomas very justly pointed out that many tumors of the uterine wall can be removed by avulsion and enu-cleation—per vaginam—and that, when practicable, this operation was much safer than either of the ones discussed by Mr. Thornton. The whole question needs further study, especially with refer-ence to the fundamental fact, quietly *assumed* in this paper—does removal of the uterine appendages always cure fibroids and fibro-cysts of the uterus?

VIII.—"The History of Twenty-one Cases of Extra-Uterine Pregnancy Coming under Personal Observation of the Writer," by Dr. T. Gaillard Thomas.

These cases are of great interest, and the conclusions drawn, carry the weight derived from probably the largest individual ex-perience, in the country, of the subject.

Most of the cases have been published before by the author and others, but their collection is of value. The results in the twenty-one cases were :

(1) Six not operated on, two recoveries.

(2) Seven operated on, four recoveries.

(3) Two tapped—from vagina,—both died.

(4) Six treated by electricity, all recovered.

The author's conclusion, that electricity is *the* method above all, is amply warranted and is borne out by the testimony fur-nished by others, especially Dr. Rockwell, in his recent admirable paper. We are not so sure that Dr. Thomas' restriction of this method to cases prior to the end of the fourth month of preg-nancy will hold good. In the discussion, Dr. Goodell reported thirteen cases he had seen. Of these :

(1) Ten were operated on, three recoveries.

(2) Three were not operated on, all died. Truly, not a brilliant record for laparotomy !

IX.—"Electricity in Extra-Uterine Pregnancy," by Dr. Henry J. Garrigues, is a compilation of eleven cases, four being identi-cal with four in Dr. Thomas' series, the only new one—of Dr. Garrigues' himself—must be regarded as of somewhat doubtful authenticity, because the diagnosis was not confirmed by any one else.

X.—"The Influence of the Constant Use of High-heeled French Shoes upon the Health and Form of the Fe-male, and upon the Relations of the Pelvic Organs," by Dr. SAMUEL C. BUSEY. This paper is charmingly written, and

seems eminently sound from the orthopedist's point of view ; but from the gynecological stand-point the author's deductions, while they seem reasonable, must be regarded—until definite evidence is adduced—as " not proven."

XI.—" The Mechanical Therapeutics of Versions and Flexions of the Uterus," by Dr. Ely Van de Warker. This is a most able analytical study of a most difficult and obscure subject. The author begins by stating two problems, which he tries to solve in this paper :

1. " What are the limits imposed by the uterus and its append-ages upon the mechanical agencies acting upon it ? "—in other words, what should we expect *not* to do by the use of a pessary ?

2. " The action of the mechanical forces under these limita-tions," or *how* do the pessaries in common use accomplish their results ?

Under the first of these headings he proceeds to show that (*a*) a certain amount of uterine mobility must be allowed ; (*b*) that the vagina imposes definite mechanical limits to the action of any pessary ; (*c*) that pessaries should not injure the soft parts ; (*d*) nor interfere with the function of any of the pelvic contents.

Under the second heading he classifies all the more commonly employed pessaries—over one hundred are figured,—and makes three groups :

1. Pessaries combined with external support.

2. Pessaries acting wholly intra-vaginally.

3. Pessaries acting within the body of the uterus—intra-uter-ine stems.

Each of these groups is subdivided, and the author then takes up each subdivision separately and the individual pessaries belong to it. While reserving his final conclusions for a subsequent paper, the author shows in his incidental comments such a clear, critical faculty that we shall eagerly look forward to his next contribution to this subject.

His general condemnation of group one, in most cases, and that portion of group three which calls for an external sup-port, in all cases, seems eminently sound. No less admirable, we think, is his analysis of the second group ; but it will be much less readily assented to, especially his demonstration of the pulley—not the lever—action of a Hodge retroversion pessary. Any one who will master the premises so fully and logically here set forth will be in a position to estimate, at their value, the some-what pessimistic remarks recently made by Dr. Matthews Duncan on the subject of pessaries.

XII.—"A New Method of Operation for the Relief of Rupture of the Perineum thro' the Sphincter and Rectum," by Dr. J. Collins Warren. The author proposes to dissect up the perineal flaps before uniting them by sutures, and claims to lessen the tension on the recto-vaginal septum, and thereby get a better anal orifice.

XIII.—"Measurements of the Uterine Cavity in Childbed," by Dr. Wm. L. Richardson, gives a table of two hundred and sixteen cases.

XIV.—"Surgical Operations on the Pelvic Organs of Pregnant Women," by Dr. M. D. Mann, is an interesting collection of cases, many of them new. The author's conclusions are that these operations are much less dangerous than are generally supposed—those on the rectum excepted. More clinical facts are needed before any very positive propositions can be safely formulated.

XV.—"Hyperæmia of the Vesico-Urethral Membrane," by Dr. W. H. Baker, is a most painstaking contribution to a little-understood subject. He reports five cases all cured by the establishment of a temporary vesico-vaginal fistula, after the diagnosis had been made by means of Skene's endoscope. The high value placed by the author on this means of diagnosis is in marked contrast to the views enunciated by Dr. Emmet in his address, and we presume that Dr. Emmet would have used his "button-hole method," if these cases had come under his care.

A graceful tribute to the memory of the late Professor James Platt White, of Buffalo, by Dr. Thomas, concludes the volume.

Looking at this volume as a whole, it must be confessed that it is somewhat disappointing. The great reputations enjoyed by many of the members of this Society would naturally lead us to look to its Proceedings for their best efforts—their most recent as well as their weightiest opinions. But in this we are not gratified; the papers, with a few exceptions, are not up to the standard such a Society might attain, while the discussions fall distinctly below what we had a right to expect in critical value.

[W. M. T.]

Diseases of the Brain and Spinal Cord. By DAVID DRUMMOND, M.A., M.D. London : Henry Kimpton, 1883, 8vo, pp. 374.

In so short a treatise, printed in coarse type, Dr. Drummond has attempted to present a summary of our knowledge of the diseases of the central nervous system, and, considering the extremely limited space at his command, he has been fairly success-

ful. The busy practitioner or student who desires to seize the salient features of these affections can here do so in a few minutes. Still it must be said that the knowledge thus acquired will be only fragmentary and provokingly abbreviated. This defect, that of over-condensation, is more evident in sections on "diagnosis," and in treatment. As a rule, the morbid anatomy and symptoms of the various affections are clearly and sufficiently stated.

The work is divided into two parts: one on the brain, the other on the spinal cord; each part opening with a fairly good account of the anatomy and physiology of the organs. With respect to the brain it may be noted that its vascular supply (areas) and cortical centres receive due attention. In the account of the cerebral convolutions, the paracentral lobule is omitted. The cerebellum is designated as "perhaps the most important organ of co-ordination in the brain," a statement which extirpation experiments in animals scarcely warrant. Nearly four pages are devoted to the electrical reaction of paralyzed muscles, and the altered "formulæ" are well given, but it seems to us that the author does not lay enough stress upon the slow, wave-like contractions so characteristic of the reaction of degeneration. The account of convulsive attacks is very good. Choked disc as a sign of brain disease is well described, the author rightly saying that the use of the ophthalmoscope "is a *sine qua non* in nervous diseases." We cannot, however, endorse his statement that "the pupil should be well dilated with atropine before a positive opinion is formed as to the condition of the disc and retinal vessels." This procedure, so annoying to the patient, is, we believe, needed in only a small number of cases, as with the upright image these parts can be well seen through a medium or slightly dilated pupil.

The article on cerebral hemorrhage is marred by the use of the term "apoplexy" as a synonym, thus diverting it from its proper use of designating a symptom-group irrespective of its pathology. The section on embolism and thrombosis is also good; but a couple of temperature charts would have much assisted the student for differential diagnosis. The subjects of hyperæmia and anæmia of the brain are treated in a moderate and conservative manner, in contrast to the exaggeration of the frequency of these affections which mars some other works on nervous diseases. We can endorse the author's statement that "anæmia of the brain is one of the commonest causes of headache." The hydrocephaloid state of Marshall Hall is described; and the statement made that choked disc may be looked for in severe cerebral anæmia. The

author believes that the state of the cerebral circulation is indicated or reflected by that of the retina—a statement which not all observers will accept.　Other good articles are those on cerebral syphilis (the author under-estimates the frequency of cerebral lesions in the first year of the infection), tumors, and general paralysis of the insane, though in the summary of the pathological anatomy of the last no mention is made of the most striking morbid histological appearance, that of the so-called spider-cells throughout the cerebral mass.　Dr. Drummond does not believe that any case of general paralysis has ever been cured ; and he evidently inclines to the view that syphilis is one of the factors of the production of this terrible disease.　Chronic abscess of the brain is pretty fully treated and due stress laid on the extraordinary remissions which occasionally interrupt the fatal course of this disease.　In the treatment of cerebral syphilis, nothing is said of the necessity of giving large doses of iodide of potassium.　Dr. Drummond has met with "mental disturbances" much more frequently in cases of intracranial tumor than we have, and he apparently exaggerates the frequency of choked discs in this affection.　Three interesting cases are briefly narrated in illustration of this section.　Hyoscyamine is recommended in the treatment of general paralysis during its stage of excitement, and ⅛ grain is given as a safe initial dose—but the student is not warned that this does not refer to the crystallized hyoscyamine which is six or eight times stronger than the amorphous.　Epilepsy has a considerable section devoted to it, but chorea, hysteria, tetanus, and hydrophobia are left out, though they are very probably connected with functional or organic changes in the central nervous system.

The structure and functions of the spinal cord are fairly well though briefly set forth, and the various diseases of this organ are described in a little over one hundred pages.　The account of ataxia is good ; the author thinks Erb's statistical statement of syphilis as a cause of the disease is extreme ; he believes that most cases are caused by cold and exposure acting upon a depressed nervous system.

The wood-cuts, especially the anatomical ones, are very rough and even indistinct.

On the whole, it seems to us that this attempt at conciseness is unsatisfactory because of lack of details, especially with reference to treatment ; we have certainly passed the time when the pathology of the most important department of medicine can be so condensed.　　　　　　　　　　　　　　[E. C. S.]

Archives of Medicine.

Original Articles.

A PRELIMINARY PAPER UPON THE CAUSES OF LATERAL CURVATURE OF THE SPINE; WITH THE REPORT OF A CASE OF LATERAL DEFORMITY DUE TO OCCUPATION.

By NEWTON M. SHAFFER, M.D.,

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THE great variety of opinions and theories that exist regarding the etiology and pathology of lateral curvature of the spine, with and without rotation, leads me to place upon record the following case. It presents many interesting points, and illustrates a form of curvature which, I think, has not been fully described, although the causes which produce it, in this individual case, are those most frequently cited by various eminent writers on the subject of scoliosis.

Lateral curvature of the spine has been written upon by so many eminent men, and the subject has been so frequently reviewed, that it may be well, before recording the case that will form the basis of my remarks, to state that I have nowhere seen that lateral curvature has been subjected to the close scrutiny which many other conditions have undergone. I regard this fact as indicative of the lax way in which all strictly objective conditions are regarded. If true scoliosis were accompanied by pain or by extensive destruc-

tion of tissue, or by any serious complication, I feel that
the causes producing it would have received the attention
which they deserve. It is only fifty years ago, for example,
that club-foot escaped from the realm of the charlatan to
the domain of scientific surgery. Within a few years only
has knock-knee received the surgical as well as the mechani-
cal attention which it requires, and, although true scoliosis
received much attention several years ago, and especially
from the French Academy, and many muscles were divided
in the attempt to make myotomy answer, in this condition,
the purpose of tenotomy in club-foot, the attempt was long
since abandoned. True scoliosis, therefore, has little to
recommend it to the general or operative surgeon. The
requirements of its treatment are almost wholly mechanical,
and the most intricate problems in its etiology and pathology
still remain unsolved.

Therefore, in dealing with this subject wherein there is
so much uncertainty regarding the pathology, and the
etiology of which is so obscure, I have thought it well to
study the matter from a clinical standpoint until such time
as actual post-mortem exploration shall settle certain vexed
questions, and, in so doing, I have been able to clearly de-
fine eight different varieties of lateral curvature of the spine,
each with a distinct etiology, and with easily demonstrated
clinical features that should enable the careful observer to
differentiate between the several forms.

The varieties are as follows :

1. The true scoliosis, with rotation of the vertebræ.

2. The hysterical imitation of the true curve.

3. The incidental curve due to an unequal length of the
lower extremities, etc.

4. The curve due to unilateral lung or pleural disease—
first described by Dr. Little, of London.[1]

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[1] "On Spinal Weakness and Spinal Curvatures." By W. J. Little, M.D.
Longmans, Green, & Co., London, 1868.

5. The curve, with exaggerated rotation, due to infantile paralysis—first described, I believe, by the writer.[1]

6. The lateral curve, which accompanies the first stage of Pott's disease in the lower and especially the last lumbar vertebræ. To this may be added the lateral deviation that may occur in any case of chronic spondylitis.

7. The rachitic curve ; and

8. The curve due to an atonic condition of the fibrous and muscular tissues in the adolescent when prolonged mal-position is maintained, as a matter of occupation especially.

I have had ample opportunities for observation in all of these conditions except the last-named, and the object of this paper is to place upon record a case of lateral curvature due to weakened tissues and habitual mal-position, and to point out the clinical features that characterize that con-dition.

John L., aged seventeen, born in Ireland and residing in New York, presented for examination at the N. Y. Orthopædic Dispensary on Feb. 4, 1884. The hereditary history shows that his father died of phthisis ; his mother is living, and seemed, when the patient was examined, in good health. One sister of the patient has hip-joint disease, one brother is now suffering from inflammatory rheumatism ; two brothers are dead from causes unknown. No other facts bearing on heredity could be ascertained.

The patient is a very poorly nourished lad ; he has a slight amount of ectropion ; his lips are large, thick, and almost color-less ; and his nutrition is generally bad. Two years ago, the patient, then fifteen, and apparently straight, entered the service of a grocer in this city as an errand or delivery boy. His employer did not use a horse and wagon for delivery, and all goods delivered were carried by this boy in a basket. The work was laborious and the hours long. In order to facilitate the carrying of the numerous parcels entrusted to him, many of which were

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[1] " Pott's Disease, its Pathology and Mechanical Treatment, with Remarks on Rotary Lateral Curvature." By Newton M. Shaffer, M.D. G. P. Putnam's Sons, N. Y., 1879 ; also, a Lecture on Lateral Curvature of the Spine. By the same author. *N. Y. Medical Gazette*, April 2, 1881.

very heavy, the patient habitually threw his body forward and to one side, hooking his right arm through the handle of his basket, and supported his burden upon the right ilium ; he was in this position a good deal of the time, and, after working steadily for about seventeen months, he began to experience pain in the "right hip" (iliac region), and his friends noticed that he was becoming crooked.   As his wages, however, were the chief dependence of his mother's family, he continued his work, disregarding the pain and the increasing mal-position.   The pain began to increase, and to trouble him somewhat when he arose in the morning, or upon changing his posture after a period of rest ; he found that he could not sleep upon his right side on account of the pain so caused ; he began to limp, "favoring" the right leg, and as the pain and mal-position increased, he was at length obliged to abandon his work and to seek medical advice.

He first consulted a physician who thought he might have hip-joint disease and referred him to Dr. I. Adler, who eliminated hip disease and sent the patient to the Dispensary.   Upon examination, it was found that the pain was referred to several places: 1st, in the back (dorso-lumbar region) ; 2dly, to the right ilium and especially at the crest, posteriorly, where he had borne his burdens ; also in the region of the hip ; and 3dly, along the course of the sciatic nerve on the right side.   The pain was always "in the back" simply, or "in the hip."   None of our inquiries gave rise to any more exact or explicit statement.   There were no points of tenderness along the spine ; pressure giving negative results. There was no evidence of chronic spondylitis, sacro-iliac disease, or morbus coxarius.   The spine was normally flexible antero-posteriorly.   There was no marked lateral resistance on either side in the dorsal region, and a pronounced lateral curvature (see engraving) with a very marked prominence of the right ilium, posteriorly. The last was a marked feature of the case.   On a casual examination it seemed almost as if there were a bony growth at the posterior iliac spine—which impression was strengthened, if any thing, by comparison and palpation.

After repeating the tests above mentioned, with the same results I directed the patient to lie down in the prone position.   The iliac prominence was then greatly modified, and the spinal curvature much reduced.   Gentle traction was then made, one assistant pulling at the axillæ and another at the ankles.   After a moment's traction the curvature practically disappeared and the iliac prominence was very greatly reduced.   There was no undue rigidity of

the spine, as already mentioned, and, as stated, the application of gentle force sufficed to remove all the essential features of deformity.
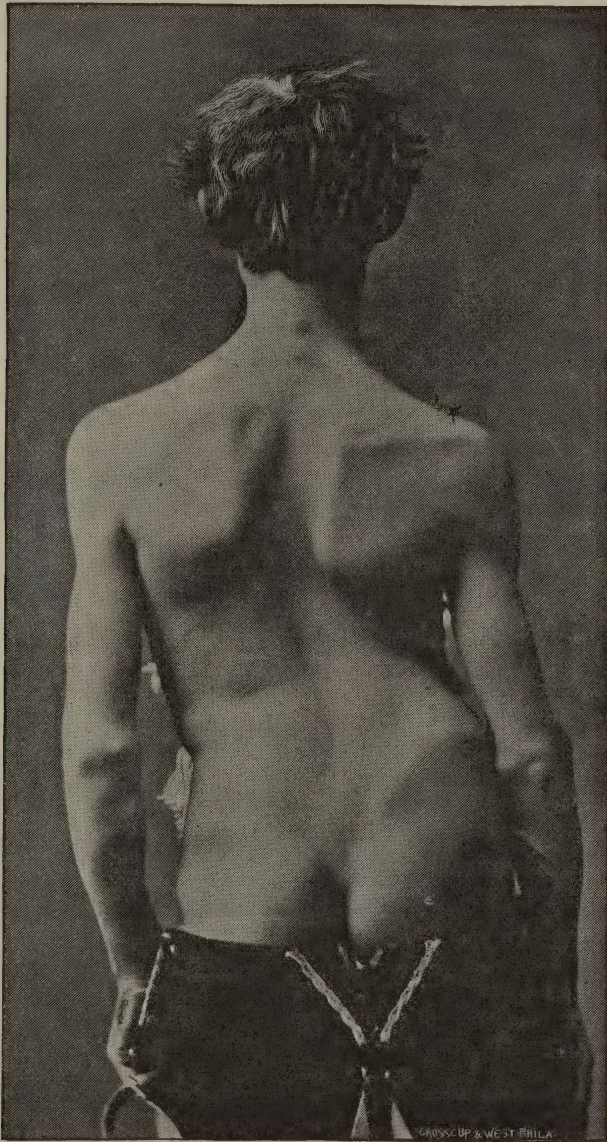
The patient was then tested as to the peculiarities of the lateral curvature of the spine. The lower extremities were of equal

length and this element of mechanical etiology was eliminated. The curve in the lower lumbar region was very short and slightly to the right ; it then passed by a long sweep to the left, forming a compensatory curve in the dorsal region. The right shoulder was depressed and there was a slight difference in the scapulæ, the left being somewhat higher. There was a very slight degree of rotation of the dorsal vertebræ toward the left side. Putting the patient to the test of extreme anterior flexion (Adams' test) it became almost straight, and there was then noticed a slight rotation at the lower (lumbar) curve, toward the right side. When the patient was placed in the prone position and traction made, the dorsal and lumbar rotation disappeared. None of these tests gave pain, and there was no evidence of reflex muscular spasm, such as is seen, for example, in chronic spondylitis or in hip-joint disease. Gravity as an etiological factor being removed, the spine, on slight traction, returned in a very brief time to a nearly normal position. All the prominent features of the deformity recurred when the patient reassumed the erect posture. The patellar tendon-reflex was slightly exaggerated on both sides, as were also the other reflexes below the diaphragm. The curve had existed for seven months, during most of which time the patient, with no improvement as to his condition and without modifying his efforts, had continued to work as described above. He was finally compelled to desist by pain and debility, and not by the increasing deformity.

Had this lad been engaged in an occupation that necessitated prolonged standing, instead of walking with his spine distorted, he would probably have developed adolescent knock-knee or flat-foot,—so frequently seen among boys of his age. Be that as it may, he came to us with a deformed spine which presented the features described, and which we have attempted to show in the engraving.

Among the many causes that have been assigned for lateral curvature—the true scoliosis of growing girls with rotation—there is none that is oftener dwelt upon, perhaps, than habitual mal-position. The patient, for example, has been observed to stand habitually, with the weight of the body supported by one limb ; or the posture of the subject

at the school-desk has been observed as awkward, or some-what out of normal ; and these or similar causes have been accepted as the important etiological factors in true scolio-sis. My friend, Dr. W. J. Little, of London, has very re-cently stated his views upon this subject, and, as they may be accepted as representing a large class of readers and thinkers, it may be well to state them here. Dr. Little says, in his excellent and exhaustive treatise on "The Medical and Surgical Aspects of In-knee (Genu Valgum [1])," (page 18,) speaking of knock-knee: "In adolescents, it is probable that carrying heavy weights, fatigue, and long hours of work have their principal share in its production, favored in fast-growing lads by insufficient diet and con-sequent weakness of tissue." Again, page 99, he says : "Nature or gravity works in the lower extremities exactly as in the spinal colum afflicted with scoliosis or twisting of the spine, which, we have already stated, is a strictly analo-gous distortion to in-knee, occurring in distinct forms,—the atonic and rachitic."

The case we have placed upon record would sustain these views, if the history so recorded were the history obtained in true scoliosis with rotation. The present is, however, a very exceptional case in our experience, but, so far as it goes, it corroborates the views of Dr. Little, as above quoted, and no one will deny, I think, that the conditions named by him may produce lateral curvature. Instead of resulting in true scoliosis, however, we have the condition, aptly called by Dr. Ketch, of the Dispensary, "a grocer boy's back."

My friend, Dr. Seguin, has also recently verbally de-scribed to me a condition which may be called an "office boy's back," wherein the deformity resulted from carrying bank or other heavy ledgers, etc., and, while it would be

[1] London : Longmans, Green, & Co., 1882.

impracticable in this paper to point out the differences be-
tween this curvature and the other curves, to which we
have alluded in our classification, it might be well to state
briefly the chief differences betweeen the "grocer boy's
back," and that found, for example, in growing girls with
rotation.

In my monograph on Pott's disease already referred to,
I call attention to the fact that permanent lateral curvature
with rotation does not result from inequality in the length
of the lower extremities, after such disabling conditions as
morbus coxarius, and infantile paralysis of long standing.
I have had, since the monograph was written, opportunities
to observe many cases of adolescent girls with suppurative
hip-joint disease, wherein the affected limbs have been
shortened practically from three to five inches or more. In
these cases, the prolonged suppuration produced just that
atony of the fibrous and muscular tissues that Dr. Little
and others place so much stress upon, and the habitual mal-
position (also prolonged) was always present when the
patients stood or walked. None of these cases have devel-
oped any thing more than the incidental curve (the third of
my classification). All evidences of curvature disappeared
in these patients when they were placed in the prone posi-
tion. Most of them complained, as did John L., of pain
somewhere in the back. It has appeared to the writer,
therefore, that even atony with prolonged mal-position is
not a very frequent cause of true lateral curvature with ro-
tation. The cases are exceptional, I think, wherein these
causes produce a permanent curve, *i. e.*, one not obliterated
by recumbency, or, as in the case of John L., by recumbency
and slight traction.

And this brings us to speak of the principal features in
true lateral curvature with rotation. There are many mat-
ters that we cannot touch upon on this occasion, and we

will refer only to those involved in this case. The first is the permanency of the curve—and the permanency of the rotation, even if slight in true scoliosis. If we place the patient in the position of extreme anterior flexion, with even a slight curve and slight rotation, instructing the patient to attempt to touch the toes with her fingers without bending the knees, the curve remains the same, or is slightly increased, and the rotation is exaggerated. In the case of John L., the lateral element almost wholly disappeared during this test, as did the dorsal rotation. The rotation in the lumbar region, which was slight, and out of all proportion to the curve, became slightly exaggerated, as was to be expected, as this was the point of true curvature, those above being secondary.

If we place a patient with true scoliosis in the prone position, the antero-posterior curves become modified, as do also the compensatory lateral ones, but the true lateral and rotary elements do not alter even with traction. In other words, we have a structural change in the muscles in true rotary lateral curvature, with, of course, changes in the vertebral bones, which, I think, are secondary. In the case of John L., there is an atonic condition only, without, I believe, structural change in the muscular or fibrous tissues. In true scoliosis there is a permanent, progressive curve, involving especially the *intrinsic* vertebral muscles, while in the case here recorded there seems to be *extrinsic* disturbance only. Ether given to the true scoliotic would fail to modify the curve; if administered to John L. it would have removed the curve at once (inasmuch as recumbency and slight traction did so), though it had existed for seven months or more. In the former condition there is a permanent loss of equilibrium between antagonistic muscles; in the latter the disturbance is almost entirely, if not wholly, functional.

Let me ask those who accept and teach the mechanical

etiology of scoliosis, why it is that the curve and twisting occur most frequently in the mid-dorsal region . The thorax forms a pedestal for extensive extrinsic muscular action,—and none of these extrinsic muscles are in a position to produce a thoracic or vertebral deformity, unless their distal extremity becomes a fixed point. Again, there is scarcely any motion at the first five dorsal vertebræ, and the remaining ones—down to the tenth dorsal vertebra— also possess very little mobility. The location of these ten vertebræ, their costal as well as their ligamentous and muscular attachments, the shape of their inter-vertebral discs, their almost interlocking spinous processes, their vertical articular processes, and their situation as regards the centre of gravity (which passes anteriorly to the bodies, in the mid-dorsal region, in the erect position),—all tend to make this the region of the least mobility in the whole spinal column. If the primary curvature were to occur in this region from gravity, would it not be a *posterior* one, as in chronic spondylitis,—this being the normal direction of the spinal curve at this point? And would not the curve occur, if gravity were the factor, at the point of the least resistance, rather than at the point of the greatest resistance—in other words, between the thorax and the pelvis, as it did in the case here recorded ? And yet, true scoliosis, with twisting, occurs most frequently in this mid-dorsal region, where there are so many obstacles to its occurrence. To make gravity, with predisposing atonic conditions, accountable for a curve which finds its principal expression at this doubly locked region, seems to the writer wholly untenable.

In the case I have recorded the cause, or causes, are not hard to find. They exactly accord with Dr. Little's graphic description of the cause of adolescent knock-knee. There are many reasons why Dr. Little's etiology, as applied to

the development of true scoliosis, should not be accepted. And I have ventured to place the case of John L. upon record as an illustration of this position.

I would like very much to take one step more in this connection, and dwell at length upon what I have come to think are the causes of lateral curvature. I am obliged, however, to desist, both on account of lack of time, and because I am now engaged in making certain tests and in conducting some experiments bearing upon the subject, which are still incomplete. In the lecture already referred to, I have said : " There is, undoubtedly, a localized or central neural lesion, which probably is developed early in life " in this condition. I can only reaffirm this conclusion on this occasion.

IODOFORM IN DIABETES.

A REPORT OF TWO CASES SHOWING COINCIDENT DIMINUTION OF
SUGAR AND OF UREA TOGETHER WITH AN INCREASE IN
WEIGHT ; ALSO AND MORE ESPECIALLY SHOWING THAT TOXIC
DOSES OF THE DRUG ARE UNNECESSARY AND, FURTHER,
THAT THEY ARE PREJUDICIAL TO ITS THERAPEUTIC ACTION.

By SARA E. POST, M.D.

THE subjects of the following experiment presented the symptoms of diabetes mellitus in a pronounced but moderate degree. Increase in the quantity of urine with an increase of sugar and of urea, together with a commencing diminution of weight, is to be noticed in their history.

Case 1 was a woman of forty-eight years. She had been married for ten years. Previous to her marriage she was healthy. She has been, at different intervals during the past five years an out-patient of the New York Infirmary. No record of her illnesses can be found, but she is said to have been treated for specific disease, and tradition gives to her a profuse pustular eruption about three years ago. At the date of my first observation, she presented no story of an initial lesion, and she had no enlarged glands. Her hair, however, was scanty, and she had frontal redness, swelling, and tenderness with intractable fissures in the angles of the mouth. She was put upon the iodide of potassium, .60 t. i. d. Even at this time she complained principally of sleepiness during the day, with broken and irregular rest at night. May 21st, four weeks later, she had not been relieved of these symptoms and she had added to them a pruritus vulvæ. Examination revealed swollen labia with general eczema of the surround-

ing parts. Sugar was found in the urine, the odor of which was bad. The iodide was continued with a special reference to the diabetic state. After five days the sugar showed increase rather than diminution. The iodide was stopped, and for the four suc-ceeding days no medicine at all was given. The sugar during these four days continued steadily to mount. By short steps of pro-gression it increased from 151 grms. to 165 grms. for twenty-four hours. At his point, upon June 1st, 1883, the iodoform treatment was commenced.

Case 2, was a woman of sixty years. She had a history of financial reverses and of family anxiety and care. Though upon her presentation at the clinic she still weighed 190 pounds, she said that she had " failed," that previously she had weighed much more. In her case also a vulvar eczema directed attention to the disease. For three weeks previous to the commencement of medi-cation the urine in this case was observed for the determination of her average excretions per day. Her quantities were found to be comparatively constant, the sugar oscillating in the neighbor-hood of 65 grms. per 24 hours. Iodoform was first given in this case August 13, 1883. Neither of these cases were restricted in diet. Case 1 lived principally on bread and tea, while case 2 had the usual diet of the middle class. In neither case was the appetite voracious, still it was very good. Thirst was not marked.

The details of our experiment upon these patients are to be found in the table which follows. In general terms it may be said that iodoform was given to case 1 from June 1st to June 16th, and again from August 13th to November 5th. Her history, therefore, presents two periods of medi-cation which are separated by two months. To case 2 iodoform was given from August 13th to September 10th; again from October 15th to December 17th, and again from January 7th to January 28th. Her history, therefore, pre-sents three periods of medication, the first separated from the second by one month, and the second separated from the third by three weeks. It may be stated further that the therapeutic influence of the drug was purposely over-stepped in its administration.

CASE I. 1st Series. Date.	Quantity of urine in 24 hours.	Specific gravity of urine.	Quantity of sugar per 1000.	Total quantity of sugar.	Quantity of urea per 1000.	Total quantity of urea.	Quantity of iodoform taken in 24 hours.	Patient's weight.	Other symptoms.	Additional treatment.
May 22.		1037	34							K. iod.=.60 grm. t.i.d.
" 23.		1033	32							"
" 24.	3750 cc.	1028	28.6	107						"
" 25.	3500 "	1029	32	112						"
" 26.	3250 "	1031	33	107						"
" 27.	4250 "	1031	35	148.7						"
" 28.	4875 "	1031	32	156						
" 29.	4000 "	1034	38	152						
" 31.	4375 "	1031	33	144			1 grm.			
June 1.	5020 "	1031	33	165	10.4	52.2	1 "		Color and odor improved.	
" 2.	3500 "	1029	30	105			1 "			
" 4.	4110 "	1028	32	131			2 "			
" 5.	4420 "	1022	21.7	95.9			2 "			
" 6.	3550 "	1026	26	92.3			1 "			
" 7.	2725 "	1021	23.7	64.5			1 "		Domest quarrel.	
" 8.	2975 "	1025	26	77.3			0.66 "		Diarrhœa.	
" 9.	5250 "	1022	22.7	119			2.0 "		"	
" 10.							2.0 "			
" 11.							2.0 "			
" 12.	4450 "	1018	16.7	75.9			2.0 "			
" 13.	3500 "	1018	18	63			2.0 "			
" 14.	2780 "	1020	20	55.6			2 "			
" 15.	2185 "	1023	26	56.8			1 "		Vomiting & diarrhœa.	
" 16.	2400 "	1014	11	26.4			1 "		Anæmia & pustular eruption.	Tinct. ferri chlor., .30 grm. every 2 hrs.
CASE I. 2d Series. Date.										
July 10.	1900 "	1019	20.8	39.5					Purplish boils over body and limbs in l'ge numbers.	
" 12.	1575 "	1020	21.7	34						
" 14.	5500 "	1016	16.7	91.8						
" 17.		1026	27.7							Tinct. ferri chlor., 1. grm. 3 times a day.
" 19.	4400 "	1032	29.4	129					Morally excited.	"
" 21.	6000 "	1020	17	102					Vomiting.	"
" 24.		1012	10.8						Diarrhœa.	"
" 26.	4100 "	1020	20	82					"	"
" 28.	4500 "	1020	21	94.5					"	"
Aug. 2.		1020	20						"	"
" 6.	3000 "	1026	22.7	68					Feels well	Reduced iron, .30 grm. 3 times a day.
" 13.	2200 "	1030	23	50.6	13	28.6	0.66 "	63 kilos.	"	"
" 14.							0.66 "		"	"

CASE I. 2d Series— cont'd. Date.	Quantity of urine in 24 hours.	Specific gravity of urine.	Quantity of sugar per 1000.	Total quantity of sugar.	Quantity of urea per 1000.	Total quantity of urea.	Quantity of iodoform taken in 24 hours.	Patient's weight.	Additional symptoms.	Additional treatment.
Aug. 15.							0.66 "		Feels well	Reduced iron, 0.30 grm. 3 times a day. "
" 16.							0.66 "		"	"
" 17.							0.66 "		"	"
" 18.							0.66 "		"	"
" 19.							0.66 "		"	"
" 20.	2550 cc.	1009	9	22.9	9	22.9	0.66 "		"	"
" 27.	2000 "	1015	16	32	11	22	0.66grm.		"	"
Sept. 4.	2500 "	1020	17	42.5	10	25	0.66 "			"
" 10.		1023	22			12	0.66 "	64 kilos.	Diarrhœa.	"
" 17.	1600 "	1022	21.7	34.7	15	24	1.3 "			"
" 24.	2250 "	1025	23	51.7	12	27	1.3 "		Weak and trembling. Diarrhœa.	Iron stop'd on account of diarrhœa
Oct. 1.	1440 "	1030	29	41.7					"	
" 8.	1440 "	1021	19	27.3	14	20		63 kilos.	"	
" 15.	1440 "	1031	29	41.7	13	18.7	1.3 "		Pale and nervous.	
" 22.	1920 "	1022	16	30.7	20.8	39.9	1.3 "			
" 29.	1440 "	1030	33	47.5	13	18.7	1.3 "		"	
Nov. 5.	2500 "	1031	35	87.5	14	35	1.3 "			

CASE II. 1st Series. Date.	Quantity of urine in 24 hours.	Specific gravity of urine.	Quantity of sugar per 1000.	Total quantity of sugar.	Quantity of urea per 1000.	Total quantity of urea.	Quantity of iodoform taken in 24 hours.	Albumen in urine.	Patient's weight.	Other symptoms.	Additional treatment.
July 12.	2640 cc.	1026	25	66							
" 14.	3120 "	1030	27.7	86.4							
" 17.	3230 "	1024	17.9	57.8							
" 19.	2640 "	1030	27	71							
" 21.	2400 "	1030	27	64.8							
" 24.	2160 "	1030	27	58							
" 26.	1860 "	1030	27	50							
Aug. 2.	2884 "	1028	27	77.8							
" 4.	2400 "	1030	29	69.6							
" 6.	2040 "	1030	29	59							
" 9.	2640 "	1030	29	76.7							
" 11.	2880 "	1029	27	77.7			Grms.	Consid-erable.	Kilos.		
" 13.	2280 "	1037	30	68.4	20	45.6	0.66	Less.	87		0.30 grm. redc'd iron.
" 14.	2520 "	1038	31	77.0	22	55.4	0.66	"			"
" 15.	2400 "	1036	29	69.6	17	40.8	0.69	Trace.			"
" 16.	2880 "	1040	33	95	18	51.8	0.66	"			"
" 17.	2160 "	1035	27	58.3	15	32.4	0.66	"			"
" 18.	3120 "	1032	25	78	13	40.3	0.66	"			"
" 20.	3360 "	1030	22.7	76.2	14	47	0.66	"		Eruc'tions.	"
" 21.	2280 "	1040	31	70.6	24	54.7	0.66	"		Ap'tite po'r	"
" 22.	2280 "	1035	30	68.4	19	43.3	0.66	"		"	"
" 23.	2640 "	1021	23.9	63	14.5	38.2	0.66	"		"	"
" 24.	2160 "	1033	20.8	44.9	25.5	55	0.66	"		"	"
" 25.	2640 "	1020	20.9	55	14	36.9	0.66	"		"	"
" 27.	2520 "	1032	21.8	54.9	18	45.3	0.66	"		"	"
" 28.	2640 "	1020	21.8	57.5	13	34.3	0.66			"	"
" 29.	2280 "	1022	16	36.4	13	29.4	0.66			"	

CASE II. 1st Series —cont.'d Date.	Quantity of urine in 24 hours.	Specific gravity of urine.	Quantity of sugar per 1000.	Total quantity of sugar.	Quantity of urea per 1000.	Total quantity of urea.	Quantity of iodoform taken in 24 hours.	Patient's weight.	Other symptoms.	Additional treatment.
Sept. 3.	2160 cc.	1030	28	60.4	20	43	6.66grm.		Trembl'g.	
" 10.	2040 "	1027	23	46.9	16	32.6	2.0 "	83 kilos.	"	Tinct. ferri chlor., 1. grm. 3 times a day.
" 17.	2760 "	1025	23	63.5	14	38.6			"	"
" 24.	2400 "	1025	33	79	10	24			"	"
Oct. 1	3120 "	1028	27.7	86.4					"	"
" 8.	2640 "	1027	29	76.5	15	39.6				"
CASE II. 2d Series. Date.										
Oct. 15.	2280 "	1030	31	70.6	15.5	34.2	1.3 "		Feels well	"
" 22.	2640 "	1025	22	58.0	15	39.6	1.3 "		"	"
" 29.	2160 "	1026	26	56	14	30	1.3 "		"	"
Nov. 5.	1920 "	1026	25	48	15	29.6	1.3 "		"	"
" 12.	2160 "	1028	22	47.5	20	43	1.3 "		"	"
" 19.	1560 "	1025	26	40.5	11	17	1.3 "		Trembl'g.	"
" 26.	2280 "	1020	16	36.4	13	29.6	0.33 "		"	"
Dec. 2.	1560 "	1025	25	39	16	24.9	0.33 "	86 kilos.	"	"
" 10.	1800 "	1029	28	50.4	16	28.8	0.33 "		"	"
" 17.	2160 "	1030	28	60.4	13	28.8	0.33 "		Sleepy & trembling.	"
CASE II. 3d Series. Date.										
Jan. 7.	2040 "	1029	29	59	8	16.3			"	"
" 14.	2400 "	1028	27.7	66.4	18	43	1.3 "	84 kilos.	Sleepiness gone.	"
" 17.	1800 "	1024	19	34	6	10.8	1.3 "		Feels well	"
" 21.	1800 "	1021	15	27	9.7	17.4	1.30 "		"	"
" 24.	2040 "	1025	25	51	16	32.6	1.30 "		"	"
" 26.	1800 "	1031	29	52	14	25	0.66 "		Paresis of low'r extremities, lasting 10 minutes, fol'wd by diar'hœa. Many of her teeth are loose.	"
" 28.	1800 "	1020	18	32.4	10.7	19.2	0.66 "	82 kilos.		"

For the chemical work which is here represented we are indebted to Miss Chevalier, Lecturer upon Chemistry in the Woman's Medical College of the New York Infirmary.

The weekly variations in these quantities are represented in the chart. This weekly determination was made by averaging the daily results during that period, with the exception that when medication was commenced in the middle of the week, only the results which followed its commencement were employed. The chart presents a view of the tables which is moderate. Both higher and lower points than are there represented were obtained. In general terms, the chart pictures only the persistent effect; the evanescent effects, as well as the spontaneous oscillations of the disease, tend to be excluded from its record.

Following the chart, case 1 will be found with an average of 140 grms. of sugar per twenty-four hours when she commenced the iodoform treatment. A high average had been maintained for at least two weeks previous to this time. One gramme of the medicine was given per day, finely powdered and enclosed in sealed capsules, .33 grm. or 5 grains being taken after each meal. The medication was commenced in the middle of the second week represented upon the chart. The quantity of sugar did not immediately fall; on the contrary, a slight elevation occurred. During the following week, however, it reached the average of 88 grms., and the week after it descended still further to the average of 55 grms. in 24 hours. During this week the dose of iodoform was increased to 2 grms. or to ten grains three times a day. Vomiting and diarrhœa occurred with a pronounced anæmia and a pustular eruption upon the skin. The iodoform was discontinued, the diarrhœa received treatment, and so soon as it could be borne, tr. ferri chloridi was given in five-drop doses every two hours. One month later the anæmia was less marked, but purplish, painful boils were scattered over the body and limbs in large numbers. Tr. ferri chloridi was again ordered in fifteen-drop doses three times a day. The patient was at this time in-

volved in domestic quarrels, she was agitated, and she
again had diarrhœa, with sugar averaging 115 grms. per
24 hours. The patient improved, the boils disappeared,
and the sugar again fell to 68 grms. per 24 hours.
August 13th, two months after the first effort, iodoform
was again tried, this time in doses of .66 grm. or ten grains
per day, five grains being taken in the morning, and five
grains being taken at night. Eructation was not com-
plained of by this patient, and her breath was not particu-
larly offensive. She bore this dose very well, and the sugar
diminished through two consecutive weeks, reaching an
average of 30 grms. per 24 hours. The average then com-
menced to become higher, though true toxic symptoms
were not developed until the fourth week, when diarrhœa
occurred. During these four weeks the patient gained
weight. This patient was subject to spontaneous diarrhœa.
The iron was stopped, and the iodoform was continued to
test more fully its effect. It was now given in doses ten
grains night and morning, or 1.3 grms. per day, and it was
continued for two more weeks. The quantity of sugar
during the first week descended to an average of 34 grms.
per 24 hours. During the second week it began ascending.
Diarrhœa and trembling also occurred. The iodoform was
stopped for two weeks, and the line fell during the whole
period, reaching an average of 28 grms. per 24 hours. The
treatment was recommenced in the 1.30 doses, and the
sugar, with some oscillation, rose, reaching an average of 87
grms. per 24 hours, the highest point of the second period
of medication. The quantity of urea also rose during this
time from an average of 25 grms. to an average of 40 grms.
per 24 hours. This period covered four weeks, and during
the whole time the patient was suffering from nervous and
intestinal disorders. The kilogramme of weight which had
been gained during the previous month was lost.

If we exclude from our consideration all oscillations which do not persist through at least two weeks, the chart presents only three well-marked lines of descent in the sugar record of this patient, the line 1–2 representing a descent from 140 grms. to 65 grms. per twenty-four hours, obtained during iodoform medication; the line 3–4 representing a descent from 65 grms. to an average of 30 grms. per twenty-four hours obtained during iodoform medication; and the line 5–6 representing a descent from an average of 50 grms. per twenty-four hours, to an average of 28 grms. per twenty-four hours, obtained during an intermission to its use. These lines are further distinguished by their occurrence during periods which are not marked with the toxic symptoms of the drug. An elevation of the sugar ratio during toxæmia, is shown in the line which intervenes between 3–4 and 5–6, also in the remainder of the series. We may then say that in case 1 diminution of sugar was associated with the presence of the drug, and with the absence of the drug following its toxæmia, also that increase of sugar was associated with the toxæmia of the drug.

Case 2 commenced taking iodoform upon the fifth week, which is represented in the chart. For the month previous the quantity of sugar had been almost stationary at 65 grms. per twenty-four hours. During the week when the treatment was commenced, the urea averaged 50 grms. per twenty-four hours. The difference in the urea averages in these two · cases results principally from the difference in their diet. As quoted by Voit,[1] the urea-averages for a man are: 15.41 grms. per day on non-albuminous diet; 22.48 grms. per day on vegetable diet; 32.50 grms. per day on mixed diet, and 53.20 grms. per day on strictly animal food. The diet of case 1 approached very nearly to the non-nitrogenous diet of this experiment, while the diet of case 2 approached the

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[1] Voit : *Hermann's Hdb. der Physiologie*, Bd. vi., Th. 1, S. 105.

mixed diet of this experiment. Taking the question of
diet into consideration, both cases present in about equal
proportions the exaggerated urea excretion of the disease.
Iodoform was given to case 2 at this time during five weeks.
During four weeks, in doses of five grains, night and morn-
ing, or .66 grms. per day. During the first two weeks the
quantity of sugar presented a primary increase, which is
suggestive of the increase which preceded the descent 1–2
in the first series of case 1. During the third and fourth
weeks of treatment, diminution of sugar to the average
quantity of 45 grms. occurred. During the fifth week the
dose of iodoform was carried up to two grms. per day. The
trembling, which was already present during the fourth
week, increased, and during the fifth and following weeks,
the quantity of sugar steadily rose, reaching an average of
88 grms. per twenty-four hours. In the third week of the
intermission toxic symptoms disappeared, and the sugar
commenced to descend. The patient felt well, and iodo-
form was re-commenced in doses of ten grains night and
morning, 1.30 grms. per day. This dose was well borne.
For five weeks the sugar steadily diminished, and there was
an increase of three kilogrms. in weight. During the fifth
week trembling appeared. The dose of iodoform was re-
duced to .33 grm., or five grains per day, but the trembling
persisted and sleepiness occurred, while both the sugar
and the urea increased. The treatment was then suspended
for three weeks. The treatment was resumed January 7,
1884, with a dose of ten grains night and morning, or 1.30
grms. per day. The response, this time, was immediate and
rapid, but, the patient complaining of eructation, the quan-
tity was reduced to .66 grm., taken in one dose, at night. In
spite of the reduction, trembling occurred during this week.
This "trembling" was choreiform in general appearance,
and the twitching of single muscles could sometimes be ob-

served. During this first week of the diminished dose, the sugar, and the urea also, was increased. At the commencement of the second week the patient had an attack, which was described in the following words : During the afternoon she was said to have commenced to feel " heavy." She was ironing, and, while at work, her arm became " powerless." About seven o'clock, she " fell upon the floor " ; she " was not faint," but " her legs appeared to give out." This condition lasted about five minutes ; ten minutes after she was able to walk. She went to bed as usual and slept, but was awakened early by a desire for stool, and between seven and ten o'clock she had about seven watery evacuations smelling strongly of the drug. She continued the ten-grain doses for the remainder of the week, felt very well, and the sugar and the urea began to diminish—as shown upon the chart. At the end of this period body-weight was found to be less, though intervening oscillations may have occurred. Many of her teeth were now found to be loosened, though she had not previously complained of salivation at any time. It is notable, that the albuminuria of this patient improved, and the question arises, whether the symptom was not produced by a cystitis which existed in the case.

During the first series case 2 presented two well-marked descents, the line included between 7 and 8 and the line included between 9 and 10. Both were associated with the use of the drug. The first lasted only two weeks, in this respect resembling the descents of case 1, while the second covered a period of eight weeks. Each of these lines of descent is followed by ascents which were, as in case 1, associated with the toxæmia of the drug. The second series of case 2 is unique in the prompt response to the medication and in the rapid and serious character of its toxæmia. It is also notable that toxic symptoms of so grave a character

* Toxic symptoms.
† Digestive and emotional disorders apparently unconnected with the medication.

should have been relieved by a diarrhœa, without the discontinuance of the drug. Case 2 took the tincture of iron during the whole course of her treatment. Anæmia did not appear. The vulvar eczema improved. General itching was not complained of in either case. In both cases the tendency to maintain the level of reduction may be noted. Two months intervened between the first and second periods of medication in case 1, yet, notwithstanding an intervening illness, the sugar was found at the commencement of the second series nearly at the level to which it had been reduced. Also between the second and third series in case 2 three weeks intervened; and it is significant that after this intermission the quantity of sugar was found not at the level which it had had before medication, but at very nearly the level at which it had been left.

Of both cases, then, it may be said that diminution of sugar and urea with an increase of weight occurred during the early part of the exhibition of the drug, that increase of sugar and of urea with diminution of the weight occurred in both cases during its toxæmia, that diminution of sugar and urea occurred spontaneously in one case after the toxæmia upon the discontinuance of the drug, also that the levels of reduction tended to be returned to, and to be maintained. In concluding the history of these cases, it may be said that they have migrated from our dispensary, not, however, on account of alarm or of dissatisfaction with their treatment. But case 1, on account of supposed personal neglect; and case 2, on account of the absorbing character of her household affairs. Both are known to be in their usual health.

The treatment of diabetes by iodoform was first proposed by Moleschott,[1] of Rome. Moleschott reported results upon five cases, as follows: case 1, a man sixty-three years, quant. of urine $2\frac{1}{2}$ litres, spec. gravity 1017, whole quantity

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[1] Moleschott : *Wiener Med. Wochschr.* Nov. 17, 18, and 19, 1882.

of sugar 19.4 grms. Case 2, a man forty yrs., specific grav-
ity 1017, quantities not known. Case 3, specific gravity
1033, whole quantity of sugar 14.4 grms. Case 4, a man
forty-six yrs., spec. gravity 1026, whole quantity of sugar 28
grms. Case 5, a man sixty-two yrs, maximum quantity of
sugar 9.2 grms. Moleschott's doses varied between .10 and
.30 grm. per day. The drug was combined with cumarin
in order to disguise its odor, and was given in pill form.
The sugar totally disappeared in case 1 with twelve days,
treatment. Of the second case it is said that the sugar was
found absent at the end of six months. In the other cases
the sugar did not entirely disappear. In the third case the
sugar had diminished from 14.4 to 1.6 grms. in three months.
In the fourth case it had diminished from 28 grms. to 1.6
grms. in four months. In the fifth case the diminution was
from 9.2 grms. to 6.1 grms.

Moleschott's formula is as follows :

R̥   Iodoformi,   1.
      Ext. Lactuc. Sat.,   1.
      Cumarinæ,   0.1
Ft. in 20 pills.   Cover with mucilage.

Dr. Moleschott had no unpleasant nor hurtful results.
In considering the value of his conclusions it is to be re-
gretted that the lactucarium was not omitted from his dose.

During what is called the iodoform debate before the
Gesellschaft der Aertzte in Vienna, Prof. Drasch [1] presented
a report of three cases treated according to Moleschott's
method. The first patient was a woman of fifty-eight years.
Her daily quantity of urine was 4 litres, specific gravity
1035, and her quantity of sugar 8 per cent. Her diet was
restricted, and iodoform .1–.3 grms. per day was given, with
cumarin, in pill form. The thirst became less during the
first week. The urine during that period sank to 2 litres

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[1] Drasch : *Wien. Med. Presse*, 1882, xxiii, 1487.

per day, and the sugar to 4 per cent. During the third and fourth weeks the whole quantity of urine was 2–4 litres, and the sugar was 2–4 per cent. After thirty days the sugar had not yet entirely disappeared. The patient had phthisis, and a hæmoptysis caused interruption in the iodoform treatment. The patient eleven days afterward died. At the autopsy an intense iodoform odor was obtained upon opening the skull. This odor was present in the lungs and in the abdominal viscera, but to a less degree. During the treatment the patient suffered with abdominal pain, and a more or less profuse diarrhœa, with a burning sensation of the fundament. She had eructations of the medicine, which were disagreeable, and which injured her appetite. She had in addition a torturing general pruritus, which was also ascribed to the drug. During the four weeks of treatment she lost 3 kilogrms. in weight. The second case had at the beginning 2 litres of urine, and sugar 1.96 per-cent. Iodoform was given in doses of .1–.3 grms. per day during five weeks. The thirst was diminished upon the second day. At the end of the five weeks the quantity of urine was $1\frac{1}{2}$ litres per day, and the sugar amounted only to .43 per cent. The sugar did not entirely disappear. This patient also suffered with itching of the skin. The third case had 10 litres of urine per day, with sugar 6.8 per cent. Upon the first day of iodoform the thirst was diminished. After four days, however, the patient returned with so intense a pruritus that he scratched himself night and day until the blood came. The iodoform was stopped. Doctor Drasch concludes that iodoform does affect the thirst and the quantity of urine with the per cent. of sugar in diabetes mellitus, especially during the first days of its use. Also that the effect upon the sugar is more constant than that upon the quantity of the urine. He adds, however, that the itching, the diminished appetite, and the diarrhœa, suf-

ficiently contra-indicate its application in the greater proportion of cases of the disease.

Bozzoli[1] says that De Renzi has experimented in Naples with good effect from iodoform in diabetes. Bozzoli himself reports two cases. In one case complete suspension of the glycosuria was produced, and in the other, a more grave case, a considerable diminution was obtained. He gives no details in regard to the cases. His dose was 1–2 grms. iodoform per day. He notes anæmia from its use.

Still more recently Sýlvester[2] of Parma reports an unsuccessful case. The patient was a man of sixty-three years. His illness was of three years' duration. Quantity of urine, 3 litres, and quantity of sugar, 132.80 grms. This patient was placed on a meat diet April 19th. April 27th, the total quantity of sugar was 124.88 grms. Continuing the animal diet, .30 grm. iodoform was given per day in six pills. May 1st, the total quantity of sugar was 103.10. The treatment was continued. May 7th, the patient refused the meat diet, but iodoform was continued. May 12th, the sugar total was 178.86 grms., and the patient continuing to grow worse the medication was suspended. Sylvester concluded that the transitory diminution was entirely due to the diet. He does not think that the iodoform had any or but very little influence upon his patient.

Doctor Drasch has given salicylic acid for the sake of comparison with the iodoform results. He gave it in doses of 3.–15. grms. per day. He found that it did not affect the sugar so quickly as iodoform, while its incidental effects were quite as disagreeable. Moleschott also compares iodoform with salicylic acid. He considers it quicker and more certain.

In our own record the descents 1–2 and 3–4, also 7–8,

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[1] Bozzoli : *Gaz. degli Ospitali Milan*, Feb. 4, 1883.
[2] Sylvester : *La France médicale*, Oct. 23, 1883.

9–10, and 11–12, were produced during the exhibition of
iodoform. On account of previous elevation in case 1 and
on account of previous constancy in case 2, these descents
are supposed to be due to the specific action of the drug.
Compared with Moleschott's, our own cases were of a grave
character, approaching to those of Prof. Drasch in degree.
Prof. Drasch notes for his cases diminution of the thirst, of
the quantity, and of the sugar, without the complete disap-
pearance of the diabetic symptoms. He also notes that his
results were most marked during the early part of the treat-
ment. In our own cases the descents 1–2, 3–4, 7–8, and
11–12 are included within the first two weeks of the medi-
cation. While during the production of the descents 3–4 and
9–10 body weight increased, during the production of the
descent 7–8 the body weight was diminished. Prof. Drasch
notes for his case of diminished weight great digestive dis-
turbance. The coincident diminution of urea during the
period covered by the descent 7–8 in our own case shows
that the loss of body weight was not due to excessive
waste, and we also have ascribed it to the lack of appe-
tite which was observed. When the weight increased during
the production of the descents 3–4 and 9–10, the medication
was well borne, the appetite was good, and the patients
were in a comfortable state of health. The primary in-
crease of sugar obtained at the point 1 and at the point 9,
have not been previously observed.

To resume: in a moderately grave form of diabetes, both
in Prof. Drasch's cases and in our own, iodoform has dimin-
ished the sugar; has diminished the urea, and has increased
the weight in our own cases; and has diminished the weight
with a diminution of sugar, in both sets of cases, where the
appetite was impaired. The therapeutic action was not
progressive but was limited to the early part of the treat-
ment in both sets of cases. This close agreement between

our own therapeutic results and those of this eminent clini-
cian is presented as a basis for confidence not only in our
lines of descent, but also, and still further as a basis for con-
fidence in our lines of ascent also or the influence of the
iodoform toxæmia, which has never yet been considered in
this connection. Quite recently Gaetano Rummo [1] has, in
the laboratory of Vulpian, very thoroughly investigated the
physiological action of this drug. Previous authors are
agreed in finding three sets of phenomena to follow its admin-
istration: first, gastro-intestinal troubles, anorexia, eructa-
tions, epigastric pain, vomiting, and frequently diarrhœa ;
second, cardio-vascular troubles, pulse 130–140, irregular, and
sometimes feeble ; and third, nervous troubles, psychic ex-
citement, agitation, hallucinations, tendency to suicide, im-
pulsive mania, and furious delirium followed by depression
characterized by melancholia, with a tendency to somno-
lence, loss of memory, and a comatose state. Some authors
find an elevation of temperature, others not. Rummo gave
to two diabetics 2 grm. of iodoform per day for twelve
days and found slowing of the pulse instead of augmenta-
tion, while the wave of blood elevated the wall of the ar-
tery in an energetic way. Patients complained of a
persistent inclination to sleep, and afterward of insomnia,
of a diminution of hunger, and of headache. Upon frogs
Rummo observed marked slowing one to two hours after
the injection of .02 grm. under the skin. At the end of
three and a half hours the heart had stopped in systole.
He says that only very large doses cause primary rapidity
and irregularity ; that, as a rule, the rhythm is regular and
the contractions are strong. Rummo remarks that if we
excise the heart poisoned by chloroform, its movements,
when resumed, have their previous character. The iodoform
heart, on the contrary, though previously extremely slow,

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[1] Rummo : *Arch. de Physiologie*, Aug., Sept., and Oct., 1883.

will, after the removal, commence to beat in a normal way. He says also that after complete cessation contractions can be re-excited by the continuous current, that the slowing of iodoform is not abolished by atropine, and that if the vagi be previously cut iodoform produces no notable action upon the heart. Rummo concludes therefore that its action is effected through the vagi by stimulation of their centres in the brain.

In dogs primary augmentation of the arterial tension occurred. In frogs observation of the interdigital membrane showed at the commencement of absorption dilatation of the capillaries, but later a progressive contraction was present.[1]

This author finds also that iodoform in poisonous doses does affect the temperature, first increasing and then diminishing it. He gave 6 grms. to a dog. The temperature was previously 39.6° C.; three hours after it was 40.2°; at the end of twenty-four hours it was 37°; and upon the following day it was 33.5° in spite of the advent of tetanus. Rummo considers its influence to be upon the heat centres in the brain.

In reference to the nervous symptoms, Rummo experimented upon the frog and deducted first a general depression, and later a general exaltation of nerve function. He gave .03–.04 subcutaneously in the posterior extremity. Locally the muscle lost its electro-contractility. If the quantity injected were great, this loss was complete. If the injection were directly upon a nerve, the nerve lost its excitability. The general effect commenced one half to one hour after the injection. The animal became less agile; it

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[1] Balp has found diminished arterial tension with the invasion of anæmia (*Gaz. degli Ospitali*, Feb. 4, 1883.) Bozzoli connects the therapeutic action of the idoform with the anæmia and the diminished tension. Such a dependence does not appear probable, as in our own cases the diminution of the diabetic symptoms was still to be observed when the anæmia was prevented by the administration of iron.

could not remove from the dorsal decubitus. Electrization of the cord or of the peripheral end of the divided sciatic showed no diminution in excitability, and the muscular irritability was intact. The sensibility and the reflexes were good. The feebleness, however, was progressive, the member which was injected being the one which was the most rapidly impaired. Movements on galvanizing the leg became less pronounced than upon a healthy frog. The irritability of the muscle was less affected than was the conductivity of the nerve. The sensation was diminished, but less than the motility. The nerves were affected more than the muscles, and the motor more than the sensory nerves. This group of phenomena embrace the first period or that of depression. Later, contractures occurred in the leg which was injected, and these contractures progressively invaded the entire body. In the muscles, which were tonically contracted, fibrillar twitchings could be observed ; also isolated contractions, now here and now there, occurred. These isolated contractions would move now a limb, and again a side of the abdomen or breast. The reflexes at this time were augmented, but to a less degree than in the poisoning of strychnine. The rigidity continued after the section of the cervical cord. 3–4 grms. were given to dogs by the stomach, or by peritoneal injection. In dogs the first period was shown by profound sleep, a tendency to avoid light and sound, general feebleness, and anæsthesia— general, but not intense. The reflexes were somewhat diminished. The pupils were contracted but responsive to light. The faradic excitability of the cortical motor area was diminished, the animal having been previously trephined. The second period was ushered in by spasmodic paraplegia. Later the anterior members also became contracted. The animal walked upon the claws of these members, and at the same time crossed his paws one over or be-

fore the other. Trembling followed intentional movement. The reflexes were exaggerated, and spontaneous and pro- voked trepidation was present. Later the animal could not progress upon the four legs, but supported himself upon the anterior extremities while he dragged the posterior ex- tremities behind him. The general sensibility was but little modified, and the intelligence was preserved. The pupils were somewhat dilated. Permanent erection with œdema of the prepuce was present in several instances. Still later, the tetanic contractions having become general, intense opisthotonos occurred. There was increased aug- mentation of the reflex excitability. All excitement, light, sound, electric, mechanic, and thermic augmented the spasms. The animal tended to rotate on its antero-posterior axis. Respiration became difficult, and the pupils were largely dilated. Transverse section of the cord did not prevent the convulsions, artificial respiration being main- tained ; and the enfeeblement was not prevented by ligature, which deprived the part of blood.

For the effect upon the digestive tract Rummo experi- mented upon dogs. For gastric ingestion the iodoform was combined with mucilage or oil. If to the dog 1.–.50 grms. were given at a single dose, or .30–.40 grms. daily for several days, the animal ate as usual; he did not vomit, but he showed great thirst. If 2–3 grms. were given at one time, or 1 grm. for several days, the dog refused solid food, but still accepted liquids. Sometimes there were vomiting and liquid evacuations from this dose. The stools were yellow and sometimes contained mucus and blood. A dose of 4–5 grms. caused the animal to refuse aliment. It vomited. It would try to drink, but even the water would not be retained. The evacuations were frequent, and resembled those of diarrhœa, and the animal became rap- idly emaciated. The gastro-intestinal trouble appeared

only to a less degree when the drug was injected into the cavity of the peritoneum. Medium doses could produce this disturbance without any sign of local irritation of the mucous membrane of the canal. In other cases hyperæmia of the duodenum would be present, and the small intestine contained much mucus. In still larger doses the hyper-æmia extended through the entire canal.

Nasal catarrh and eruption of the iodic type occurred.

The saliva, the bile, the urine, the pancreatic secretions, were increased. The secretions were obtained by means of canulæ, and upon the curarized dog, artificial respiration being maintained. In all instances it was noted that when the iodine reaction appeared in the submaxillary gland, the secretion was increased, the saliva and the bile becoming three times their usual quantity. If iodoform were given by the mouth, the iodine reaction appeared at the end of one hour to one and one half hours after its injection. If the medicine was dissolved in ether and injected under the skin, the iodine reaction appeared in ten or twenty minutes after its injection. The augmentation, as a rule, lasted only about forty minutes. During this time the quantity increased rapidly, and then diminished slowly until the normal was reached. The intestinal secretion was found to be increased, ocular demonstration being made upon the curarized dog. All the secretions were natural in character, except the bile, which was less viscid and less colored than is usual. Hypersecretion of mucus frequently occurred. The urine was at first increased. It sometimes contained albumen. When albumen appeared in the urine, the elimination of the iodoform was diminished.

In general terms it may be said that according to these observations iodoform does specifically affect the secretory, the nervous, and the circulatory functions. Iodoform is not locally irritating. When taken by the mouth, it does not

induce inflammation in the gastro-intestinal canal. The secretions are increased, especially the secretion of the salivary, of the gastro-intestinal glands, and of the liver. The effect upon the nervous system is first upon the nerve centres, and later upon the cord and upon the nerve trunks, upon the motor rather than upon the sensory tract. There is first produced diminution of voluntary motility, and later a diminution of the excitability of nerve trunks. The first stage of this effect is depressive. Later exaggerated reflexes, permanent contractures, and tonic convulsions occur.

The physiological action of iodoform had been previously considered, notably by Bouchardat, by Righini, by Maitre, by Binz, and by Högyes. Rummo's deductions do not materially conflict with these authors,[1] while his results are fuller and better defined. In our own cases the diarrhœa, the trembling, the agitation, the paresis, and the loosened teeth all find their counterpart in Rummo's dogs. The diarrhœa is explained as a hypersecretion similar to that which is obtained from the purgative salts.[2] The pulse in our cases was observed only for quality, which always remained good. Concerning the nervous symptoms in our cases, the twitchings in case 2 and the restlessness and agitation in case 1 could not be called signs of depression. The stupor and paresis appeared only once. If we accept the fact of the precedence of depression over excitement, we shall be obliged to conclude either that the primary stage may fail to appear or that their exists a still further preliminary stage. Reference to our records will show that upon the invasion of any one of these symptoms the diminution of sugar was lost, and that if the medication were persisted in an actual increase of its excretion did occur. That this increase was not incidental is shown by the fact that upon the omission of the drug diminution

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[1] Nothnagel and Rossbach : '' Hdb. der Arzneimittellehre,'' 1880, S. 343.

[2] Dr. Matthew Hay: *Jour. Anat. and Phys.*, July, 1883.

again would occur. Such a descent after toxæmia is shown in the line 5–6 of the chart. The ascent of sugar sometimes preceded the toxic symptoms mentioned. This is shown in the ascent which follows 3–4, and in that which follows 11–12. Based upon the above, we have ventured upon the proposition that in diabetes an increased excretion of sugar accompanies the iodoform toxæmia, and that, therefore, toxic doses of iodoform are unnecessary in diabetes, and, further, that they are prejudicial to its therapeutic action. The existence of this principle can be traced not only through our own experiment, but also through all the other therapeutic efforts which have been reported. Moleschott had no toxic effects. Drasch mentions diarrhœa and abdominal pain, but he also mentions that the therapeutic action of the iodoform was limited to the first few days of its use. Of Sylvester's case it is said that the patient refused food. Iodoform has been given in phthisis pulmonalis first by Semola[1] and later by Möller.[2] Semola gave it in divided doses of .05–.50 grms. per day. He says that the hectic was diminished. He does not speak of unpleasant effects. Möller gave it to eleven patients in doses of .10 grm. per day. He notices improvement in weight, which was not progressive, and which covered only the first few days.

Compared with the quantities given by Moleschott, by Drasch, by Semola, and by Möller, our own doses were large. We adopted the doses of Bozzoli, whose paper we first read. The continuance of the medication is a factor of equal consequence with the dose. The single toxic dose of iodoform for a man is very large: 50 to 80 grms. according to Högyes.[3] Baruch[4] has used 6 to 10 grms. at one time

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[1] Semola: *Allg. Wiener med. Zeitung*, July 25, 1882.
[2] Möller: *Wien. med. Presse*, 1883, S. 1675.
[3] Nothnagel and Rossbach: " Hdb. der Arzneimittellehre," 1880, p. 343.
[4] Baruch: *N. Y. Med. Jour.*, Jan. 5, 1884.

in the puerperal uterine cavity without injury, and Rummo has found absorption from such a denuded surface very good. Prof. Dittel,[1] in the Vienna debate, said that some German physicians used 400 to 500 grms. at one time in external applications. Toxic symptoms have usually appeared after a prolonged use. One of Oberländer's[2] cases had taken it for eighty days. The slowness of the elimination of the drug may be noted. Capranaca, who made determinations for Moleschott, found in the urine, daily, only iodides sufficient to account for a small proportion of the iodoform taken. Three days after the cessation of the medicine, iodides still existed in the urine. In one of Prof. Drasch's cases, iodine was found in the urine seventeen days after the cessation of the dose. Accumulation of the drug is to be seen in our own cases, possibly, in the response to the iodoform medication, which occurred with increasing promptness during each successive period of treatment. Case 1 commenced her first medication upon May 31st, and the diminution of sugar occurred toward the end of the second week, Her second period of medication commenced Aug. 13th, and a marked response was found upon Aug. 20th, or at the beginning of the second week. Case 2 responded to the first medication only toward the end of two weeks; to the second, within one week; and to the third, within three days. This fact, together with the fact, that in case 2 at least the toxic symptoms became with each series progressively more grave, will suggest either a surplus of iodoform in the system, or the occurrence of a structural change. The persistency of the results of the iodoform also may be remembered in this connection.

Binz[3] considered the effect of iodoform to be due to the iodine which constitutes $\frac{9}{10}$ of its weight. Rummo, on

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[1] Dittel: *Wien. med. Presse,* 1883, xxiii., S. 1487.
[2] Oberländer: *Centralblatt f. d. med. Wiss.,* 1879, S. 336.
[3] Binz: *Arch. f. exp. Path. u. Pharm.,* Bd. viii., 1877, S. 309.

the contrary, considers it to be due to the drug in its own
form. He advances, as a proof, the fact that large doses
may be absorbed by the stomach and from the peritoneum
without hyperæmia or any other sign of local disturbance ;
that iodine is not narcotic ; also that iodoform is eliminated
in its own character from the lungs. Further, the odor of
iodoform in the various organs of the cadaver might also
be mentioned. In the urine, and in the other secretions,
iodoform is eliminated as an iodide. It cannot, however,
be stored up as an iodide, because the elimination of the
iodides is rapid.

The accumulation and the persistency of this drug must
receive consideration—even in the exhibition of the small
dose. It appears to us that intermissions should be pro-
vided for, just as is done in the administration of other
cumulative medicines. According to our experience, iodo-
form can be happily administered in doses of .66–1.00
grms. per day only for about two weeks. The smaller
dose is not, as has been shown, administered for a much
longer time with benefit. Our experience has shown also,
that the level of reduction will persist through an inter-
mission of from two to four weeks, when it will be again
diminished under medication in the same ratio as before.
The full quantity of iodoform can be given in one dose
at night, by which method eructations and anorexia are
less apt to occur. It can be given in capsules or in pill
form. Rummo has found that the stomach bears it better
when dissolved in oil. Such a solution might also be put
up in capsules. We have not had, in our cases, any gastric
disturbance which depended upon the single dose ; great
care, however, was taken by dipping the capsules in
paraffine to completely close them and to confine the odor.

The pathology of disease frequently has been elucidated
by the character of its therapeutic agents. Rummo's au-

topsies showed as follows : When 2 grms. were taken by the
dog only hyperæmia of the duodenum was present. A
dose of 5–6 grms. produced hyperæmia throughout the in-
testines in disseminated patches and in longitudinal lines.
The stomach contained a yellow fluid and the intestines
diarrhœal matter. The liver was yellowed and shrunken.
The spleen presented central ecchymoses and the kidneys
were congested. Congestion was present throughout the
cerebral pia and throughout the gray substance of the brain,
of the medulla, and of the cord. The medulla was softened
and of a deep rose color. With very large doses the white
substance also was congested, and little ecchymotic points
could be found in the gray matter. On the trephined
animal the hyperæmia appeared when the animal abandoned
itself to sleep and it was most marked during the convul-
sions. For microscopic examination the animal was killed
one, two, or three days after the ingestion of the drug.
The liver was found to be the first organ affected. Rummo
says that its cells first lose their regular polyhedral shape
and become dilated and vesicular. They contain a gran-
ular semi-liquid substance, and about the interlobular
branches of the portal veins are little granules which
color with osmic acid. In a degree more advanced the
hepatic cells also contain these granules, but the peripheral
cells of the lobule contain many more than the cells of its
centre. No sign of inflammation is present. The portal ves-
sels were often dilated and contained granules of fat. The
biliary canals were not altered, except that their epithelium
often showed fatty change. The epithelium of the stomach
and of the intestines was in the same condition, also that
of the gastro-intestinal glands. The heart and vessels did
not escape. The inter-muscular, the inter-fibrillar, and
even the intra-fibrillar substance was granular. Neither the
parenchyma nor the sarcolemma showed, however, signs of

inflammation.   The kidneys were altered.   If the poisoning were slow only fatty degeneration of the cells of the tubules and of the endothelial cells of the vessels was found.   If the dose were very large, however, in the glomeruli the capillaries were dilated and an exudation more or less extensive occurred.   In some of the glomeruli it was so abundant that the blood-vessels were compressed.   In the perivascular fluid he observed yellow granules, which were probably due to the destruction of corpuscles; also lymphoid and red cells, which had probably emigrated from the vessels into the space.   The medullary region was congested with blood and presented extravasations.   The tubules showed degeneration of the epithelium and casts.   In the nervous system the most marked changes were to be found in the dorso-lumbar region of the cord.   The vessels were gorged with blood, and transudation and extravasation through rupture were found.   The anterior horns contained many newly-formed cells, while the multipolar cells were diminished in volume and had lost their processes, their nuclei were obscure, and they were transformed into little piriform and ovoid bodies.   The axis-cylinders and the sheaths were not materially altered.   The same lesions were found in the psycho-motor region of the brain.   According to these post-mortem observations iodoform would appear to attack particularly the central nervous system, the liver, and the ultimate tissue cells.   It thus equally supports the two prominent theories of the disease.   The trophic influence of iodoform suggests the theory of Pettenkofer and Voit,[1] who find the origin of diabetes in instability of the protoplasm with inability to appropriate oxygen and to effect complete oxidative change.   Pettenkofer and Voit[2] consider fatty degeneration also to be pro-

[1] Pettenkofer and Voit : *Ztshr. f. d. Biologie*, 1867, S. 380.
[2] Voit : " Hdb. der Phys.," Bd. vi, Th. 1, S. 235.

duced by cleavage in the albuminous molecule without oxidation. Iodoform toxæmia produces fatty degeneration. Logically, therefore, it should increase the diabetic symptoms. Such an increase we have found to be one of its earliest signs. If for the therapeutic action of iodoform we suppose a primary stimulation of cell-power Pettenkofer's theory is sustained.

While Rummo uses the phrase fatty degeneration in describing the changes of the iodoform toxæmia, he inclines to believe that the fat found in the cells is infiltrated from without. He also refers to the fact that Hoppe-Seyler, Ludwig, and others do not accept the theory of Pettenkofer, with the recent work of Lebedeff,[1] who has found distinctive fats stored up in the body unchanged. Further, Schiff's theory, newly advocated by Pavy,[2] finding the origin of diabetes in an abnormal activity of the liver, determined through nerve influence by a loss of vascular tone, this theory also is supported by the action of iodoform, which is said to attack the liver before it attacks any other organ, which contracts arterioles in the therapeutic dose, and whose influence upon the vagi must suggest an invasion of the diabetic centre in the brain.

To recapitulate:

1. Iodoform in doses of 1 to 2 grms. per day has diminished the sugar and the urea, and has increased the weight in two moderately severe cases of diabetes mellitus.

2. The therapeutic influence of the drug was limited to the first few weeks of its use.

3. Increase of sugar and of urea with diminution of weight accompanied the iodoform toxæmia in both cases.

4. The toxic symptoms here referred to were anæmia,

[1] Lebedeff: *Arch. f. die gesamt. Phys. d. Mensch. u. Thieren,* 1883, xxxi., S. 11.

[2] Pavy: *Brit. Med. Jour.,* May 5, 1883, p. 8.

salivation, diarrhœa, agitation, trembling, choreiform movements, sleepiness, and paresis.

To conclude: It is proposed that in the iodoform treatment of diabetes, systematic interruption to the medication should be made. With a dose of 1 grm. per twenty-four hours, the interruption may be made at the end of from one to two weeks; the intermissions may consist of from one to two weeks, and the entire quantity may be given in a single dose securely sealed and taken at night.

A PLEA FOR THE SUBSTITUTION OF THE SO-DIUM IODIDE FOR THE POTASSIUM IODIDE IN THERAPEUTICS.

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AMIDST the prevailing scepticism in therapeutics, few remedies have better maintained the right to be considered curative agents than iodide of potassium in the treatment of tertiary syphilis. So firmly is our faith in the powers of this drug established, that failure is generally taken to imply either a neglect of the element of quantity, which is as important as that of quality in the use of drugs, or to throw a serious doubt upon the correctness of the diagnosis.

All the more therefore is it to be regretted that this drug produces, in addition to its curative effects, others, which are not only non-curative, but positively disadvantageous and injurious. This is true of all drugs. We have no therapeutic agent whose effect exactly counterbalances that of the disease which it is meant to cure ; the very nature of the human organism, and the manner in which it is affected by disease, preclude even the possibility of such a finely limited action. On the contrary, even that portion of the effect of a drug which is curative remains so only within certain limits, beyond which it destroys where it was meant to cure. It is therefore the office of the therapeutist not

only to find a remedy for a certain disease, but to use it in such a manner that the ill effects will be reduced to the minimum. It is with this in view that I am led to call attention to the advantages to be obtained from the use of the sodium iodide, instead of the corresponding potassium salt.

It is well known that the administration of large or even moderate doses of the potassium iodide is very frequently followed by the annoying and sometimes alarming manifestations of the condition known as "iodism." As a result the patient and sometimes the physician is discouraged from the use of a remedy which appears almost as bad as the disease it is meant to cure. It is generally recognized that the therapeutic utility of the potassium iodide upon the general system is due to the fact that it is an iodide, and not that it is a potassium salt, and the employment of the potassium iodide, to the exclusion of all other iodides, has led to the belief that all of the injurious as well as beneficial effects of this salt are common to all iodides ; and hence that the condition known as iodism must be endured, in order that we may avail ourselves of the benefits of the iodides. That is to say, the iodides as a class have been saddled with much of the toxic action of potassium salts. We seem to forget that potassium salts, even when found amongst the iodides, are still potassium salts, and must exert a potassium action. We affirm a certain physiological action for the potassium compounds, and forget it merely because the potassium happens to have another active element combined with it, whose effects we desire, while those of the potassium are not required. How illogical it is to state that a certain dose of potassium chloride, for instance, produces certain effects, and to overlook these effects entirely in the action of even a larger dose of the potassium iodide, and yet these are the very errors of which we are guilty when we assume

that all of the undesirable effects of the potassium iodide are unavoidably connected with the use of the iodides. It might be claimed, however, that many of the beneficial effects of the potassium iodide are to some extent due to the fact that a potassium salt is used, and hence that it would be inexpedient in many cases to substitute another iodide, such as the sodium iodide, for the potassium salt. To determine this point it will be necessary to enter upon a more especial study of the therapeutic effects of the potassium iodide; what is due to the potassium nature, and what to the iodine constituent of the drug.

The principal effects produced by the administration of the potassium iodide in doses sufficiently large to give us the well-known therapeutic results can conveniently, for our purposes, be divided into two classes:

I.—Effects that are therapeutically utilizable.

II.—Effects injurious to the system and therapeutically non-utilizable.

In the first class are included (and here we shall only give those effects which have been recognized by all therapeutists):

1st.—A specific curative action upon the various manifestations of tertiary syphilis, and, in combination with mercury, upon secondary syphilis.

2d.—A reducing or wasting, vaguely termed "alterative," action upon hypertrophied lymphatic and thyroid glands.

3d.—The power of hastening the excretion of metals in chronic metallic toxæmia.

4th.—A curative power over chronic and subacute articular rheumatism and gout.

Other effects, such as a supposed curative action in non-specific, chronic, and acute diseases of the nervous system, non-specific inflammations of serous membranes, etc., I have purposely omitted. This has been done, not because the

influence of the iodide in these cases could not be accounted for on a physiological basis, for this is true even of some of the most important therapeutic applications of drugs, but because the utility of the drug in these affections does not even rest upon a firm empirical basis. In some of these conditions the iodide is used merely because no other drug is indicated, and only for the purpose of doing something.

To the second class belong all other well-observed physiological effects which occur to a greater or less extent during the therapeutic administration of the drug. These are, in the order of their toxic importance :

1st.—A weakening effect upon the heart's action, a lowering of the blood-pressure, and slowing of the pulse (these results are produced only by medium or large doses).

2d.—A catarrh of the mucous membranes of the gastrointestinal canal, resulting in loss of appetite, dyspepsia, and mal-nutrition.

3d.—A temporary catarrh of the conjunctival, nasopharyngeal, and respiratory mucous membranes.

4th.—A peculiar cutaneous eruption.

Before proceeding to review in order these various effects, we must remember that our primary object is to study them with a view of showing that the effects coming under our first category are for the most part not due to the potassium nature of the drug, but to its iodine component, and that, therefore, we might reasonably expect similar beneficial results from the use of the sodium iodide, and that the second series of effects are, to a great extent, due to the potassium element in the salt, and might be avoided by substituting the corresponding sodium salt.

First among the therapeutically utilizable effects of the iodide of potash is the one which is best known, and upon which the chief utility of the drug depends, namely, the specific effect which it exerts upon secondary and tertiary manifestations of syphilis.

In spite of the numerous theories which have been ad-
vanced to account for the marvellous power possessed by
this drug in this disease, we are still driven to the subter-
fuge of hiding our ignorance, by terming it a specific action.
But yet, both theoretically and empirically, it can be
shown that other iodides possess this power to an extent
equal to that of the potassium salt. There is, in the first
place, no other potassium compound, as such, which has
any curative power over syphilitic processes. Again, in
studying the physiological effects of the iodides, the sodium
iodide is constantly used by physiologists in their experi-
ments upon animals in preference to the potassium iodide,
whenever the drug is to be injected directly into the blood,
and always produces effects like those which the potassium
salt produces. Finally, such authorities as Nothnagel and
Rossbach assert that they have used the sodium iodide al-
most exclusively for the past years, where the drug had to
be given for a long time, with results equal to those ob-
tained from the iodide of potassium. In this connection it
will, perhaps, not be out of place to refer to a case of
my own, which is one of several undoubted syphilitics, in
which I used the sodium iodide with perfect success. The
patient was a married woman who had aborted, or given
birth, to dead children at full term upon four successive
occasions. Her husband gave an undoubted history of a
chancre two years before marriage ; the wife came under
my care for severe osteitic pains of the sternum and tibiæ ;
she also suffered from severe headaches, and an ulcer on
the upper half of the left leg, together with indolent boils
which were scattered over the whole body, and some which
had ulcerated. All the symptoms presented by this pa-
tient were removed by the internal administration of sodium
iodide, and the external application of iodoform to the ul-
cers. It appears, therefore, that from a physiological and

chemical standpoint, we are warranted in expecting the same results from the use of the sodium as from the potassium iodide in syphilis, while empirically we think some reliance should be given to the results obtained by the authorities above quoted.

The so-called "alterative" action of the potassium iodide is acknowledged by all to be due to the iodine element of the salt. Indeed, in enlarged thyroid or lymphatic glands, we combine the internal administration of the iodides with the external application of iodine to the enlarged glands. Any assimilable iodide—such as the iodide of iron—will produce the same effect as the potassium salt in these cases, and here, again, therefore, we are not restricted to the use of the latter compound, but can use the sodium iodide.

The power possessed by the potassium iodide of hastening the excretion of the metals from the system, is a firmly established one, especially for lead and mercury. Indeed, some authorities have explained the action of the iodides in tertiary syphilis, on the supposition that it causes the excretion of the mercury, which has been previously taken and stored up in the system. Of course, this theory can only be maintained by such as believe that the mercury produces the tertiary manifestations. However ridiculous such a theory may be, I refer to it here, for the purpose of showing how positively the action of the iodides, in hastening the excretion of the metals, is established. It is supposed that the iodides, when taken into the system, convert the metals deposited in the different organs, into soluble combinations, and thus enable them to be excreted from the system more rapidly. There is, certainly, no reason to suppose that the sodium iodide can not do this as well as the potassium salt; indeed, many authorities believe that the potassium salt is converted into a sodium compound in

the system, and hence it is at least as logical to administer the sodium as the potassium iodide for the purpose under discussion.

Finally, we come to consider the curative power which the iodides exert over chronic rheumatism and gout. This is accounted for by different authorities in two ways: first, on the ground that the potassium iodide in an " alterative," as are other iodides, and that it hastens tissue metamorphosis ; secondly, that it acts nearly in a manner similar to the other alkalies in rheumatism, changing the pathological condition of the blood upon which the rheumatic process depends. This theory of its action will not account for the undoubted brilliant effects which follow the administration of the iodides in this disease and which do not follow the use of the alkalies ; indeed, were we to acknowledge the validity of this theory, we should certainly prefer other alkalies to the potassium iodide. By the alterative action of potassium iodide is here meant not only the power which it has of causing the joint deposits in chronic rheumatism and gout to disappear, but also includes the supposed property of the iodide of changing the faulty nutrition and tissue metamorphosis upon which the rheumatic process depends ; but here, again, the fact that potassium iodide is changed to the sodium salt in the blood, would necessarily imply that the administration of the sodium iodide would be just as efficacious in this disease as the potassium compound.

We have thus attempted to show that, therapeutically, the sodium iodide is as valuable for most purposes as the salt now used ; but were this all, we should hardly be justified in substituting for a well-known remedy one which is still comparatively unknown. It will not be difficult, however, to give positive reasons for a preference in favor of sodium iodide.

With this view, we shall take up the second class of effects resulting from the therapeutic use of the iodides.

Here, again, it will be necessary to examine into each of these briefly, for the purpose of determining which of these are necessarily connected with the use of the iodides, and which are rather the results of the potassium action of the drug.

All physiologists and therapeutists agree that all potassium salts, whose diffusion power is sufficient to admit of their absorption, exert, in large and medium doses, a weakening effect upon the heart's action.

Mickwitz, after bringing animals (cats) under the influence of curare, and thus eliminating the element of shock from the experiment, injected 0.05 grm. of potassium nitrate into the jugular veins. He found, as a result, a diminution of the blood-pressure and slowing of the pulse,[1] .0.20 grm. even caused death by paralysis of the heart. Bunge,[2] comparing the effects produced upon warm-blooded animals (rabbits, dogs, etc.) by potassium chloride, found that it would take fifty grms. of a potassium salt, administered by the stomach, to produce paralysis of the heart in man. This quantity, however, would act as an emetic, so that fatal poisoning could scarcely be produced in this way ; but these facts will serve to show that, when the heart is already weakened from age or disease, large therapeutic doses of potassium iodide, such as alone are efficacious in some pathological conditions, would be dangerous from the effect exerted by the potassium upon the heart. On the other hand, no such injurious action upon the heart is exerted by the sodium salts (Bogolepoff). Bœhm [3] has shown that the sodium iodide exerts no deleterious effects upon the cardiac muscle or ganglia. In this respect, therefore, the sodium iodide is far preferable to the potassium salt.

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[1] Nothnagel and Rossbach : "Handb. der Arzneimittellehre," p. 15.

[2] *Zeitschr. f. Biologie,* 1873–4, vols. 9 and 10.

[3] *Archiv f. exp. Path. u. Pharm.,* v., p. 337 (1876).

Next in order we find the gastro-intestinal catarrh as one of the frequent injurious effects of even moderate doses of the drug. In some cases the presence of such a catarrh proves an effective bar against the employment of the potassium iodide, and the physician is forced to content himself with inferior remedies. An analytical study of the physiological action of the drug will show that the catarrh of the gastro-intestinal tract is due to the salt as a potassium compound, and not as an iodide. We find that when muscular tissue is cut out of the living body and placed in a solution of a potassium salt (chloride), two to three per cent. in strength, that such muscular fibres immediately lose their irritability. If this exposure to the potassium salt be not continued for too long a time, immersion in a sodium-chloride solution will restore the muscle to its irritability. And, furthermore, we find that when healthy muscular tissue, cut out of the body, is placed in a sodium-cloride solution, its irritability will be *preserved* for a much longer period even than when immersed in pure water (Kühne). The salt-frog so common in the physiological laboratory is a practical instance of the stimulating action of sodium-chloride solutions upon the living animal tissue.

Kühne has shown that while potassium-chloride salts have a paralyzing effect upon muscular and nerve tissue with which they are brought in contact, sodium solutions (chlorides) restore and preserve the irritability of nerves and muscles to which they are applied. Keeping these facts in view, we must also remember that in the gastro-intestinal canal the potassium-iodide solutions come in direct contact with the walls of the stomach and intestines, and, we have every reason to believe, with the muscular coats of these organs, although a slight amount has perhaps been already converted into sodium iodide before the drug has penetrated as deeply as the muscular layer. As a result, we

have intestinal and gastric dyspepsia, which necessitates
the discontinuance of the remedy, or if the disease impera-
tively requires it at all hazards, then the patient must
suffer from mal-nutrition as a result of the dyspepsia. Thus
the physician finds that while he is striving to cure one
pathological condition, he is substituting another none the
less serious.

Many practical therapeutists, among others Prof. Seguin,
recognizing the importance and cause of this gastric ca-
tarrh, always order the potassium iodide to be taken in
large quantities of Vichy water. I have followed this
plan, and have found that the iodide was much better
borne than when given in undiluted (often saturated) solu-
tion. This follows necessarily from the facts above stated.
I should here like to refer to the gastric catarrh produced
by the administration of the bromides. I am convinced
that here also the chief cause is the direct effect of the
potassium salt upon the gastric mucous membrane; the so-
called "bromide breath," to which the catarrh gives rise, is
not distinctive of the bromides, but is present when a sub-
acute gastric catarrh has been produced by the administra-
tion of the iodides or from other causes.

It is highly probable, then, that the most annoying and
injurious complications which follow the continued use of
large doses of potassium iodide are due to the fact that a
potassium and not a sodium salt is used.

The catarrh of the respiratory and naso-pharyngeal mu-
cous membrane, when it occurs, is probably the result of
the iodine component of the drug; this is only of minor im-
portance as far as the general health of the patient is con-
cerned, and is variously accounted for by different authori-
ties. The only theory as to its etiology which we shall
mention is the one advocated in the latest edition of Noth-
nagel and Rossbach. According to these authorities, the

catarrh of the nasal and respiratory mucous membranes occurs only when either free iodine is used externally, together with the potassium iodide internally, or when the potassium iodide which is used is impure, from the presence of some free iodine; in either case, it is the direct irritation of the free iodine, either in process of excretion, or when inhaled as a vapor, which causes the catarrh. If this be true, these effects will be avoided by care being taken to administer a pure potassium or sodium iodide.

What has been said as to the cause of this catarrh will hold good for the eruption also; the latter is the external manifestation of an irritation of the integument, just as the former is of irritation of the other mucous membranes.

Theory then being in favor of the use of the sodium iodide in preference to the potassium salt, it only remains to determine the dosage, and to state what results have already been attained by this substitution.

Since the iodine is the important constituent of both compounds, the sodium iodide ought to be more effective than the potassium salt, for equal quantities of the former contain more iodine than the latter. The atomic weight of potassium being 39.1, that of sodium 23, and that of iodine 127, there will be 127 parts of iodine in 166 parts of KI, while there are 127 parts of iodine in 150 parts of Na I. This difference in strength, however, is to some extent counterbalanced by the fact that sodium iodide is of somewhat lower diffusion power than the potassium salt. We should therefore expect, as has been found in practice, that equal doses of the two salts would give equal iodine effects.

It is claimed, then, for sodium iodide that :

1. It can be used therapeutically for almost all, certainly the chief purposes, for which potassium iodide is used, and, I believe, with similar beneficial effects.

2. That sodium iodide is more assimilable than the iodide

of potash, both locally to the digestive organs and to the general system.

3. That, as a result, many of the local and general undesirable effects which are produced by the potassium iodide, do not follow the use of the sodium iodide.

It is to be hoped, therefore, that the sodium iodide will be used by those whose clinical advantages admit of an extensive trial of the drug, so that a more extended experience may confirm that which a limited experience would seem to claim for this drug.

TRAUMATIC ANEURISM OF THE VERTEBRAL ARTERY—CURED BY COMPRESSION.

BY ROBERT F. WEIR, M.D.,

SURGEON TO THE NEW YORK HOSPITAL.

On the 8th of December, 1883, a man named Robert Adams, aged twenty-eight, was brought into my ward at the New York Hospital, having received a short time previously a stab wound in the right side of the neck, from a knife held in the left hand of his opponent, who faced him when striking at him. The patient said he had bled very freely, but his clothes were not much stained with blood, nor was he weakened or exsanguinated. When first seen by the house surgeon, no further bleeding was taking place from the wound, which was situated about three quarters of an inch below the lobe of the right ear, and just anterior to the sterno-cleido-mastoid muscle, transverse in direction and about half an inch in length. There was below this point and extending several inches downward an ovoid soft non-pulsating tumor running under and posterior to the mastoid muscle. All exploration of the wound was avoided and an iodoform dressing applied and secured by a compress and firm bandage.

December 10th.—Tumor has almost entirely subsided. Wound nearly healed. Pt. yesterday afternoon suddenly experienced almost complete paralysis of sensation and motion of the left arm and hand. This was preceded by a " queer " confused not painful feeling in his head, momentary in duration. Consciousness not lost.

December 15th.—Tumor has disappeared. Wound entirely healed. Paralysis is slowly disappearing. At a point two inches below and one inch posterior to lobe of right ear palpation discovers a faint pulsation slightly expansive in character, and on auscultation a slight bruit is audible.

December 25th.—Since the last record all the signs of an aneurism have developed at the point indicated in last note. Pressure on the carotid just below the level of thyroid cartilage does not affect the pulsation in the tumor, but pressure over the tubercle of sixth cervical vertebra controls it at once ; nor does it react as long as the compression is continued. Ice-bags and pressure were ordered to be applied alternately every three hours over the tumor.

January 1st.—The signs of aneurism, including pulsation, thrill, tumor, and bruit, are still more pronounced. No appreciable effect has followed the treatment by ice and pressure. The area of pulsation is now nearly $2\frac{1}{2}$ inches in diameter. The paralysis of the left arm is becoming less marked.

January 3d.—To-day digital pressure was resorted to at the lower anterior edge of the aneurism, where yesterday it was found that compression arrested all pulsation. This was continued for seven hours by the house staff, assisted by relays of students. Considerable force was necessary at first to control the circulation in the sac, which occasioned some pain and discomfort, and required morph. gr. $\frac{1}{3}$ hypodermically to keep the patient quiet. No cerebral symptoms occurred at any time during pressure. After two hours there was great diminution in force of pulsation, and slight pressure controlled it without further discomfort to the patient. After three hours the pulsation could not be felt, and all signs of the aneurism save the resistance due to the tumor on palpation, had disappeared. Pressure was continued lightly until 7 P. M. (in all 7 hours), at which time a firm graduated compress was applied. No cerebral symptoms were noticed during the treatment.

No return of symptoms. A solid tumor can be appreciated at site of aneurism. Paralysis of arm has almost completely disappeared. General condition excellent.

January 12th. Since last note pt. has been up and about ; no return of sign of aneurism. Only a slight trace of the swelling can now be felt. The patient was to-day discharged from the hospital as cured.

The above case so happily terminated by a bloodless procedure is interesting from the general rarity of the injury, and also from the fact of its being the only one successfully treated by compression. According to Fischer,[1] in 32 cases of injuries of the vertetral, there were only 9 cases of

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[1] Krankheiten des Halse : *Deutsche Chirurgie*, Lief. 34, 1880.

aneurism, and all of these resulted from stab wounds. The most satisfactory résumé of the few cases of vertebral aneurism on record has been given by Holmes, in his justly celebrated lectures on the " Surgical Treatment of Aneurism in its Various Forms," [1] which is herewith appended with the addition of Cattolica's second case.

The significant fact in the above collection is the error so frequently fallen into by the surgeon, in taking the injured vessel to be the carotid or one of its branches. In all these instances the carotid artery was tied without benefit, or to the detriment of the patient. The only case mentioned by Holmes of neurosis from a vertebral aneurism is that of Möbres, in which the external carotid was exposed for ligation, but on applying pressure to the denuded artery no effect was produced on the aneurism, and the operation was abandoned after the patient had refused to submit to the laying open of the tumor. Ultimately the aneurism ceased to beat under the application of cold, and disappeared. The cessation of pulsation was preceded by very severe pain.

Since the date of Mr. Holmes' paper I have been able to collect but one other case besides my own, that of Kocher,[2] which is included in Fischer's cases. This brings the number of recoveries up to three out of ten. Kocher's case was also from a stab made in a man, forty-two years old, between the fifth and sixth cervical vertebræ. Three weeks after the reception of the wound, during which time several hemorrhages occurred with the formation of a pulsating tumor, Kocher enlarged the wound and removed the laminated clots, etc. He was able to seize the bleeding point with a long forceps, but could not apply a ligature to it. He then introduced a little pledget of lint dipped in the perchloride of iron, and crowded it in the space

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[1] *Lancet,* July 26, 1873.
[2] *Arch. f. klin. Chirug.,* Bd. xii., 1871.

Surgeon.	Author.	Compression Tried or No.	Death at What Period. Post-mortem Appearances.
1. 1829, Chiari.	Filiatre Sebezia, ann. 3, fasc. 2; Chassaignac, "Traité des opérations," p. 334.	No.	Death nine days after ligation of carotid from pyæmia. Aneurism situated on the vertebral artery between the first two vertebræ.
2. 1834, Romaglia.	Ibid, and Froriep's Notizen, vol. xli., p. 89.	Compression on the carotid seemed to diminish the pulsations.	The carotid artery was exposed, and a ligature put round it, but not tightened. Died from hemorrhage. The aneurism was seated on the first curve of the vertebral artery, which in this case lay between the second and third vertebræ, instead of the first and second.
3. 1836, Cattolica.	Gaz. méd., 1836, p. 435.	No.	Wound of artery high upon the neck. Carotid tied. Death from subsequent rupture of the sac.
4. Ibid.	Ibid.	Yes, without avail.	Carotid exposed for ligation, but not tied, as pinching it was found not to stop pulsation in aneurism. Aneurism sloughed.
5. 1840, Klyskens.	Chassaignac, op. cit., p. 239.	Compression stopped the pulsation, but the ligature of the carotid did not.	Death four months afterward from hemorrhage, after the bursting of the aneurism. No post-mortem examination.
6. 1847, Northern Hospital, Liverpool.	South's Chelius, ii., 250.	No.	Died fourteen days after operation of tying the carotid from rupture of aneurism, which lay between the transverse process of the fourth and fifth cervical vertebræ.
7. 1863 (?), at St. José Hospital, Lisbon.	British Med. Journ., vol. i., 1863. p. 197.	No.	Death on twentieth day after ligature of carotid. Cause not given. Aneurism situated between second and third vertebræ.
8. 1865, Lücke.	Langenb. Archiv., vol. viii., p. 78.	Compression on the carotid diminished but did not quite stop the pulsation.	As the symptoms persisted after the ligature of the carotid, the aneurism was injected with perchloride of iron. It burst and was laid open, and the bleeding vessel commanded by pressure. He died more from the consequences of the ligature of the carotid than anything else. Vertebral wounded between atlas and occipital bone.

between the transverse processes of the vertebræ, and secured it by graduated compresses of carbolized gauze, fixing the head in an immovable position by a stiffened cravat. The patient progressed steadily to a cure.

In the two cases where the diagnosis was made, the simpler measures of digital compression and direct compression on the wounded vessel have sufficed to cure the aneurism. It is probably unnecessary to more than allude in condemnation to the proposed ligation of the vertebral either at the root of the neck, preferably from behind the sterno-mastoid, or at the space below the transverse process of the first cervical vertebra, where the tortuosity of the vessel to accommodate the free rotatory motions of the head allows it to be easily reached. Unfortunately an aneurism is most commonly located in this region, and overlaps or obscures the artery so as in all probability to render its ligation under such circumstances difficult. The plugging after exposing the injured artery has also been shown by Kuster,[1] in two cases of wound of this vessel, to be easily accomplished. For this the plug or tampon of iodoform is to be preferred.

The principal difficulty in the cases of hemorrhage or aneurism in this locality is to determine whether the carotid or its branches, or the vertebral artery is sundered. This point is to be arrived at by carefully considering the effect of pressure on the carotid not only above but also on a level with the transverse process of the sixth cervical vertebra, or rather over the anterior or so-called carotid tubercle. Compression above this point affects, as a rule, the carotid artery only ; over and below the transverse process compression will not only occlude the carotid but also the vertebral artery, which runs to its outer side. It

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[1] *Berlin klin. Wochenschr.*, Nov. 26, 1883. Allusion is made to a successful cure of aneurism by Warren, but I have not been able to obtain the details.

must be remembered, however, that the vertebral instead of entering the foramina in the transverse process of the sixth cervical vertebra in its upward course sometimes remains exposed until it enters higher foramina. To meet such possibilities it has been suggested to isolate the carotid according to Rouge's plan, which is, with the sterno-mastoid relaxed to grasp the muscle with the finger and thumb and to pinch the carotid between them. It is necessary also to keep in mind in trying to effect vertebral compression that the carotid tubercle is, according to Chassaignac, always from two to three inches above the clavicle.

A traumatic aneurism whose pulsations are unaffected by compression of the carotid applied, as above detailed, will be found to concern the vertebral artery, and should be treated first by ice and compression by bandages, or by the fingers, failing which I should deem the opening the sac, with the finger introduced to close the arterial opening un-til the fibrine clots are cleared away by the enlargement of the wound, and the subsequent firm antiseptic plugging most likely to bring about a cure in the rare cases of this injury.

STATIC ELECTRICITY IN MYALGIA.

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THE re-introduction of static electricity as a therapeutic agent has led some to its indiscriminate use in all cases in which electricity is supposed to be indicated, and this has resulted as might be expected in very frequent disappointment. It is important, therefore, that data should be collected as to the classes of cases likely to be benefited by its use, and those in which it is inapplicable or of inferior value.

It is evident that when the entire person is charged with electricity, and the discharge is effected at a given locality on the surface, the process is widely different from simply making a portion of the body a part of a circuit. In the former case the electricity passes from all directions toward the point at which the discharge takes place. It thus acts upon a large extent of tissue, the intensity of the action increasing as the above-mentioned point is approached. In the latter case the action is limited to the path of least resistance between the two poles, and there is no similar concentration of effect from all directions at the point of emergence.

All this is independent of any different quality in the agent itself. But that such a difference exists is undenia-

ble. The greater diffusibility of static electricity as com-
pared with other forms is perhaps its most striking peculi-
arity. It is difficult to confine it to a conductor, however
good, and even by the aid of the most perfect insulation.
This peculiarity can scarcely be without effect in its
therapeutic application. Instead of following exclusively
the channels of best conduction, its tendency must be to
pervade the tissues. We see how readily it passes through
non-conducting substances, such as paper, for example,
and there seems to be no good reason why it should not do
the same in the case of animal tissues.

What might be assumed *a priori* as to difference in effect
is confirmed by clinical experience, and there is no longer
any doubt that while static electricity cannot be substituted
for the other forms in all cases, there is a field in which it
is more useful than galvanism or faradism. As illustrating
its value in myalgia, the following cases are of interest:

CASE 1.—Acute lumbago. P. D., aged 42, coachman, received
a thorough wetting from a sudden shower, and sat for some time
after on the box exposed to a cutting wind. This resulted in
lumbago so severe that it was with difficulty he could get to my
office. His body was bent forward, and he supported himself by
placing his hands upon his knees. Static electricity was applied
to the lumbar region with considerable energy, nearly cutting him
in two, as he expressed it. The first application resulted in
enabling him at once to straighten up and walk erect. A second
application, on the following day, removed the remaining soreness,
and he resumed his position on the box. There was no return of
the lameness.

CASE 2.—Chronic lumbago. J. B., 'longshoreman, presented
himself October, 1883, at the clinic, at the Post-Graduate School,
with a history of lumbar myalgia, which had completely disabled
him for several months. His body was bent forward, and he
walked slowly and with great care, the slightest jar causing great
pain in the back. If he stooped down he was unable to straighten
up again without the assistance of his hands placed upon his
knees. He had been treated with liniments, plasters, etc., but

with no benefit. On the following day I applied static electricity, using nearly the full power of the machine, and continuing the application for ten minutes. When he stepped down from the insulating platform he was able to pick up an object from the floor, and to rise up again without the assistance of his hands, and with very little pain. He left my office walking erect and with a confident step. Two days afterward he returned, stating that he felt like a new man. There was still, however, some lameness and stiffness in the back which were entirely removed by two more applications.

CASE 3.—Myalgia of the muscles about the hip. John B., aged six years, who had been for some time under my care for cardiac disease, was found one morning to be unable to walk, on account of extreme pain on attempting to flex the thigh.

He was brought to my house in a carriage, and carried up the steps and into the office. On examination, I found that there was no pain when the limb was at rest or when it was moved passively, but attempts at voluntary motion caused severe pain, which was located principally in front of the hip-joint, and seemed to be the result of tension of the muscles and fascia in that region. There was slight tenderness on pressure, but no heat or swelling. Pulse and temperature normal.

Patient was placed on the insulated platform, which was connected with the positive side of the machine. Sparks of slight intensity were then drawn from the affected part by means of a small electrode applied outside the clothing. The application was borne with very little complaint, and at the end of about ten minutes the patient stepped without assistance from the platform and walked with ease about the room and to the carriage. On the following day there was a sense of weakness in the limb, but no pain or soreness.

CASE 4.—Myalgia in the shoulder and arm.

Mrs. T., aged thirty-six, had two years ago an attack of acute rheumatism affecting the knees and hips, and confining her to the bed for ten days. Since then she has had several slight attacks, mostly in the upper extremities. Early in January, 1884, the flying pains from which she had suffered more or less for some time seemed to settle in the left shoulder and arm. There was no general fever and no local heat or swelling, but a slight local tenderness. Movement of the shoulder was decidedly painful. These symptoms were entirely removed by a single, rather ener-

getic application of static electricity, and there has thus far been no return (March 8th).

CASE 5.—Myalgia of the muscles of deglutition.

Miss F., aged fifteen, came to me to be treated for a "sore-throat." For ten days she had suffered considerable pain in swallowing and a constant more or less severe aching in the throat.

Inspection of the fauces showed the mucous membrane to be in a normal condition. Pressure on the base of the tongue with a spatula gave pain, as also pressure from without above the hyoid bone. There was no heat or swelling. The case was clearly one of myalgia located in the muscles about the base of the tongue.

Several folds of a towel were wrapped about the neck, and through them sparks of moderate intensity were drawn, the sitting continuing five or six minutes. Considerable relief was immediately obtained, and two more sittings on alternate days effected a permanent cure.

CASE 6.—Acute myalgia of the intercostal and abdominal muscles. Dr. T. H. B. called at my office on the afternoon of February 29, 1884, suffering from acute pain in the left side, extending from the sixth rib to the pelvis. His body was bent toward the affected side, and he found it impossible to stand erect. He had been in that condition for about six hours, and the pain was rapidly growing worse. Static electricity applied for five or six minutes enabled him to straighten up and to leave the office in a comfortable condition, and to attend the theatre in the evening. The pain did not wholly cease until the following evening, but became less and less after the application.

CASE 7.—Chronic myalgia of one arm and knee.

Mrs. S., wife of a physician, had suffered for several months from muscular soreness in the right arm and wrist, resulting from a strain received in a fall. There was also a spot on the inside of the leg below the knee-joint where muscular action produced extreme pain, rendering it impossible to fully flex the knee. For six months she had not been able to kneel.

Three applications of static electricity on alternate days entirely removed the trouble in the arm and wrist, and so far relieved the soreness in the knee that the patient, with care, can now kneel without pain. Probably a few more sittings will result in a perfect cure.

The above are selected from a considerable number of

cases of myalgia in which I have found static electricity decidedly beneficial. Indeed in any case in which muscular action is painful, if inflammation can be excluded, I should confidently expect benefit from this agent. In purely neuralgic pain I have not been so successful in its use.

The fact that this form of electricity can be applied through the clothing is a very great recommendation on the score of convenience to the physician and of acceptability to the patient.

PRE-THYROIDAL CYST—LARYNGEAL ABSCESS AND FISTULA—CURE.

By RAMON DE LA SOTA, M.D.,

SEVILLA.

The subject of this history is J. F., a woman, twenty-six years old, single ; tall, well built ; of nervous, lymphatic constitution, without any pathological antecedents except the ordinary sufferings which are peculiar to childhood. There is no diathesis whatever in her family.

When, on December 9, 1881, I saw her for the first time, she had been suffering from her throat about two months, in consequence, she said, of a very severe excitement. This, I learned, was a quarrel that lasted over an hour, and in which she stood her ground with that impetuosity common to low-bred Andalusian women. Immediately after she took from a jar a drink of water which was very cold.

At once she experienced a hot impression in the larynx, feeling as if a foreign body was lodged in this organ. Afterward she commenced to feel some stinging pain ; a dry and frequent cough set in, and a persistent hoarseness. All these symptoms became gradually worse, at the same time that there appeared in the anterior part of the neck a lump which rapidly augmented. Breathing and deglutition had not been materially changed till the previous week ; but since, swallowing and breathing were getting more difficult every day. She often experienced an attack of suffocation of brief duration, and sometimes, on account of the severe pain in her throat, she could not swallow her food, but was forced to throw it out after having it a little while in the mouth. Except sleeping, all her other functions took place regularly ; her physical and mental sufferings prevented her from sleeping, and she was exceedingly frightened on account of her illness.

On examining the neck I found in the anterior part of the infra-hyoidal region a fluctuating tumor, of about the size of a hen's egg, not sensitive to lateral displacement, but which gave her great pain if pushed from front to back. The skin covering it had the usual color and consistency, and was not adherent to the tumor, as it glided with the greatest facility in every direction, and a fold of it could be raised everywhere.

But as the tumor moved up and down with the larynx during deglutition, and it was quite impossible to pull the two apart, there was no doubt but that it was adherent to this organ.

The patient's voice was hoarse and scarcely audible ; her cough dry and cracked ; her breathing somewhat difficult ; and when, following my directions, she swallowed saliva, her face revealed signs of acute pain.

The laryngoscopical examination showed an inflammatory state in the whole pharyngo-laryngeal mucous membrane ; but the tumefaction and the deep-red color of this membrane was most remarkable on the posterior surface of the epiglottis ; in that part, situated just under the anterior angle of the vocal cords, a swelling of the size of one half of a hazel-nut, and of a very deep-red color, could be noticed.

In the part of the neck corresponding to the lowest portion of the external tumor I introduced the needle of Pravaz' syringe, and with this instrument made aspiration, but failed to draw out any quantity of the liquid supposed to be there. Nevertheless, I persisted in believing the tumor to be a cyst of the pre-thyroideal serous bursa, as the point of the needle moved freely in every direction, instead of being held by a solid mass. Drawing out the needle, I laterally compressed the tumor, and then there came out, through the opening of the puncture, a stringy transparent liquid, very much like the white of an egg. I stopped before completely emptying the cyst, as the patient suffered a good deal, and asked me not to trouble her any more for the moment.

Three days afterward she came back to the Polyclinic with the same subjective and objective symptoms, and I repeated the compressions till the sides of the bursa were in mutual contact. On finishing, I heard a crack, and at the same moment the patient was attacked by a severe humid cough, accompanied by a slight whistling through the aperture in the neck, and she expectorated some purulent sputa, with some streaks of blood and of a very fetid odor. With the laryngoscope I observed that the subglottic tumefaction had considerably diminished. Through the cutaneous

opening I introduced a probe, that I moved about in every direction ; I touched the thyroid cartilage, which was altered and rough, and under its inferior border the probe entered the larynx, producing a severe paroxysm of cough. I then gave the probe to one of my assistants to hold, and by means of the mirror I perceived it just in the middle of the larynx, it having passed through the anterior wall precisely at the point where the infra-glottic tumefaction had been seen before.

On the 26th of December my patient made her appearance at the Polyclinic again.

The external tumor had not re-acquired its previous size ; the internal one was entirely gone and in its stead a granulating surface was seen, in the middle of which there was the internal opening of the fistulous passage going from the larynx to the skin of the neck. Out of the external aperture there was continually dropping a viscous clear liquid. There was neither dyspnœa, dysphagia, nor spontaneous pain, but the latter was provoked by coughing. The cough was not hard, and was always followed by muco-purulent expectoration. Every effort forced the air through the fistula, and then a clear whistling sound was heard. The probe introduced through the fistula passed freely into the larynx, producing a violent cough, and was immediately found in the laryngoscopical image. The pharyngo-laryngeal inflammation was not so intense as heretofore.

I passed vertically through the tumor a filiform seton, availing myself of the puncture that I had made with the needle of Pravaz' syringe on the first day, which puncture now was the external opening of the fistulous passage ; and with a small portion of salicylated cotton, soaked in a solution of chloride of zinc, I touched the diseased mucous membrane.

On the 3d of January all the subjective symptoms, with the exception of a slight pain produced by the touch, or by coughing, had disappeared. Deglutition and respiration were quite good. The voice was clear. The patient was joyful, had a fine complexion, and was getting stouter. The cavity of the cyst was greatly reduced ; it was noticed that the probe could not move in every direction as before ; the only route that it could now take being the passage of the seton and the laryngeal fistula. Through the cutaneous opening of the fistula there flowed a homogeneous pus of good character. There were only some granulations in the pharyngeal mucous membrane ; the laryngeal was entirely healthy with the exception of a small portion under the angle of

union of the vocal cords, corresponding to the internal fistulous opening, where a little tumor of the size of a small pea could be seen. The whistling sound produced by the passage of air through the fistula was still heard. The same treatment was continued.

On the 10th of January the cavity of the cyst had entirely disappeared; only with great difficulty could the probe penetrate into the larynx. I removed the seton.

During this month nothing was done but washing with carbolized water all the affected parts; but on seeing that there was no cicatrization except at the superior opening of the seton, I made at the end of January an iodined injection through the fistulous passage. The liquid entered the larynx, producing violent cough and troubling considerably the patient who, for some time, perceived a strong taste of iodine.

During the month of February the injection was twice repeated without any material benefit, and the patient's aversion to iodine became so great that on the second of March I used in its stead a strong solution of chloride of zinc; this injection was repeated once a week. Soon after, a great improvement was observed: in the month of April the internal opening disappeared; the air no longer escaped through it, and the probe could no longer be made to reach the larynx. Some exuberant granulations formed in the cutaneous wound, and, in order to destroy them, I applied to them nitrate of silver. Afterward there remained an ulcer which, a few days later, was again covered with granulations; I repeated the silver application several times, till the 26th of May, when cicatrization was complete.

One year after this I had the opportunity of seeing the patient, who was perfectly well. The only sign of her illness that had remained was a hollow scar adherent to the subcutaneous tissues.

Remarks.—The case which I have just described seems to me somewhat remarkable in its production as well as in its course and termination.

After an excited quarrel, in which the efforts of the voice were neither slight nor of short duration, a woman takes a drink of cold water and, immediately after, felt something in her throat. So far there is nothing remarkable; nor is it astonishing that a laryngitis more or less intense ensued.

But it is not what we generally observe, that the laryngitis was followed by a perichondritis, and this by a cyst of the pre-thyroidal bursa; the patient being free from any constitutional affection, as was proved by the preceding account and by the relative facility of cure. Some authors, Tobold among them, affirm that they have seen cases in which refrigeration alone has been the cause of perichondritis. I had previously always found this affection as symptomatic of some acute illness, such as typhus, small-pox, scarlet-fever, etc., or of some chronic disease such as syphilis, tuberculosis, scrofula, lepra, etc. And this is the only time that I have known it to be produced by any local and accidental cause.

The development of the cyst is yet more strange. This I consider due to the transmission of the inflammation from the perichondrium to the serous pre-thyroideal bursa, in which the phlogistic process determined such a preternatural secretion, as to rapidly fill up its cavity and distend its walls.

That the cyst was formed by an excessive functional action and not in consequence of an exudation of blood, was proved by the character of the liquid therein, so very similar to that which is found in ranula. The formation and existence of ranula Malgaigne attributes to hyper-secretion of mucous glands and not to that of serous bursæ; but this idea is inapplicable to the pre-thyroidal space.

I presume that the abscess was imperfectly opened in the cavity of the cyst between the patient's first and second visit; when, by compression, I broke it in order to completely evacuate the liquid, the pus poured out into the larynx; and the severe attack of cough which expelled it from there, forced the air to pass whistling through the narrow fistulous passage. I do not consider it probable

that the rupture simultaneously took place in the laryngeal and cyst cavities.

I have stated that not only the antecedents but the relative facility of the cure prove that the patient was entirely free from any constitutional affection. If her disease had in any way been the result of the general state of the patient, the radical cure of a morbid process so very complex could never have been attained through local treatment; at the very best, only a temporary closing of the fistula could have been obtained.

While I had this case under observation, there came to my private office a student of medicine, a brother of a distinguished professor of the University at Barcelona, who, having neglected the first periods of a constitutional syphilis, came to me with a gummy tumor in the infra-hyoidal region, which had abundantly suppurated, and finally formed a complete laryngeal fistula. By means of a vigorous general and local treatment, all the injuries in the neck and larynx entirely disappeared; the only visible signs of the fistula which afterward remained were a radiating hollow scar of a brown hue on the skin and a notable fold in the right half of the epiglottis, produced, no doubt, by the retraction of the cicatrix from the internal fistular opening. When this patient felt himself free from all his complaints, he gave up coming to my office, and thinking himself cured, he renounced all treatment. The summer of 1882 passed, and with it disappeared the supposed restoration. After a few months of continual suffering, this young man came to consult me again; his larynx was greatly injured, and the fistula open again. All my endeavors to save the life of this unfortunate person were useless; he died last April a prey to laryngo-pulmonary phthisis of a syphilitic nature.

I used the filiform seton for my patient's cyst, because in

the treatment of ranula it has always given me a satisfactory result. And having obtained also a good success in the present instance, I recommend its use, as well as the injection of the fistulous passage with a solution of chloride of zinc.

I will not conclude without stating that, as there were no alarming symptoms, and as I thought that the purulent collection was not entirely formed, I did not introduce into the larynx, under the guidance of the mirror, the lancet with concealed blade, in order to cut open the intra-laryngeal tumor, after having made the first laryngoscopical examination. But had dyspnœa indicated that the patient's life was in danger, no other consideration would have kept me from incising the laryngeal mucous membrane as far as I should have been able.

A CASE OF ACUTE ATROPHY OF LIVER—WITH MARKED DELIRIUM.

BY JOHN BOWEN, M.D.

THIS case presents no very unusual symptoms, except, probably in the violence of the delirium. Its chief interest is that it was mistaken for a case of acute mania, and thus sent to the insane asylum.

When patient was admitted to the asylum, her condition was such that she could give no history of her illness, and we were unable to get at her friends until after her death; thus, though we were satisfied that the diagnosis of acute mania, made by the two physicians certifying to her insanity, was incorrect, and that the patient had delirium of acute disease, we were unable to determine what the acute disease was.

The following history is given as it appears on the record of the asylum :

Sarah K., æt. twenty-three ; single ; born in Ireland, and has resided in New York for the past eighteen years.

Patient was admitted to the Kings County Insane Asylum in the evening of July 17, 1882.

The certificates of the physicians certifying to her insanity set forth that "she has attacks of acute mania. She raves incoherently on religious subjects, refuses to eat ; attempts to escape from the house ; has to be restrained by force, etc." On arriving at the asylum patient was calling loudly for the "Mother of God," and, as she resisted mechanically every effort

to assist or direct her, had to be carried forcibly to the ward. On reaching the ward, it was noticed that the conjunctivæ showed jaundice. Patient walked the floor in an agitated state, and called almost constantly for the "Blessed Virgin." It was impossible to gain her attention for a moment.

July 18th, morning.—I was unable last evening, by the artificial light, to determine the amount of jaundice. Patient continued to walk the floor, and with clasped hands and upturned face, kept up a constant wail for the "Virgin Mary," until a late hour when she was given a sedative of morphia and chloral. She refused food, but before going to sleep asked for and drank a glass of water. After taking sedative she slept six or seven hours. This morning patient is less agitated and inclined to be dull. She refuses food and drink, and will not permit pulse or temperature to be taken ; still impossible to gain any information from her concerning her illness.

18th, evening.—Continues to refuse food and drink. Dulness has given place to stupor, but she continues to offer forcible resistance to every effort made to examine her. Has passed urine several times, but bowels have not moved since admission. Is given a purgative. Jaundice is well marked everywhere, and has decidedly increased since admission, but still is not at all intense.

July 19th, morning.—Patient was very quiet all night. Bowels moved three times. Jaundice increasing. Stupor has become semi-coma. Makes slight efforts at emesis. Pupils equally dilated. Am able this morning, for the first time, to make physical examination, and find that patient has an easily reducible right inguinal hernia, for which she has been wearing a truss, and which appears to have given her no trouble. The area of liver dulness is somewhat diminished. Temperature, 98⅘°. Pulse, 110 and irregular in time and volume. Respirations, 30. Ten ounces of urine drawn with catheter ; and is found to be deeply stained with bile, and to contain a large amount of albumen and numerous granular casts from kidneys. Patient gives evidence of pain when pressure is made over liver.

19th, evening.—Pulse, 90 and very feeble. Temperature, 99°. Respirations, 38 and irregular. Pupils largely dilated, but partially react to light. Coma is profound. Efforts at vomiting have continued and grown more frequent, but patient has vomited nothing. Slight hemorrhage from uterus.

July 20th.—Patient died at 12.5 this morning. She gave no signs of spasm at any time.

To-day I gained the following from patient's sister :

Deceased had variola when five years old. The attack was light, and recovery complete. Since then she has not been sick enough at any time to be confined to bed, until her last illness. Two months ago deceased went to the country with the family she has been employed by for some time past, and expected to remain until October, but on the 14th, three days previous to her admission to the asylum, she made her appearance at the residence of her sister, in Brooklyn, about eight o'clock in the evening. She greeted her sister's family as usual, and after talking quietly for a while about her visit to the country, complained of feeling tired, and said that she would retire. When offered food, she declined, saying that she did not feel hungry. Up to this point deceased showed nothing peculiar, so far as noticed by my informant, but on being asked why she had returned so much earlier than was expected, she declared that her mistress had treated her so unkindly that she could not live with her longer, and that the cook had poisoned her. My informant discredits the story about the unkindness of the mistress, as deceased had lived with her for several years, and was very much liked. She declares, however, her belief in the story of the poisoning, and says she has employed a detective to go to the country and work up evidence on which she can prosecute the said cook.

Informant says that after retiring, deceased remained in bed and was quiet but sleepless until about eleven o'clock, when she suddenly sprang from the bed, threw herself upon her knees, and called loudly for the " Virgin " to save her, saying she was lost, lost ! She could not be induced to return to bed, and remained in this agitated state until the arrival of the physician at ten next morning. She was then put to bed and kept there by the use of morphia, chloroform, and mechanical restraint, until removed to the asylum. While at her sister's she neither ate nor drank any thing : passed urine several times but bowels were not moved.

My informant further states that patient began menstruating at the age of sixteen.

Informant says she has five unmarried sisters living, who, she claims, are in every way normal. She also denies that there is any disposition to neurotic trouble in her family. She is accompanied by a sister whose physiognomy and behavior gave unmistakable evidence of a defective nervous system.

When I informed these two sisters that an autopsy had been made on deceased, they both had prompt outbursts of hysteria

and heaped abuse upon me unsparingly.   When, finally, it was
suggested to them that they should be obliged to me for having
made the post-mortem, and thus removing the disgrace that would
have attached to the family name (had not the autopsy decisively
revealed that deceased was not insane), removed a serious impedi-
ment to the marriage of the remaining sisters, my visitors at once
became as apologetic as they had been abusive.

It is not believed that the inherited neurotic tendency, which
I believe the patient had, predisposed particularly to acute atro-
phy, but it may account for the early appearance and violence of
the delirium.

Post-Mortem Appearances.—Autopsy eleven hours after death.
Post-mortem change very slight ; rigor mortis entirely absent ;
body fairly well nourished.   Skin of head, neck, abdomen, and
arms considerably jaundiced, as is the sclerotic ; that of legs
slightly so.   Skull cap normal.   Vessel of dura possibly a little
engorged.   Pia normal, as also is the cerebral substance.   Lateral
ventricles contain a moderately increased amount of yellow-stained
fluid.   Numerous small ecchymoses under visceral pleura of both
lungs, and two small hemorrhagic infarctions in anterior portion of
upper lobe of left lung.   Yellow-stained mucus in bronchi.

Heart is of normal size ; valves are in good order ; large black
soft clot fills right auricle and ventricle, and a smaller one of same
description partially fills left ventricle.   There are numerous small
ecchymotic spots to be seen on the visceral layer of the pericar-
dium ;   there are numerous ecchymoses in omentum and mes-
entery, but none to be seen on peritoneum ; mesenteric glands
not enlarged.   Mucous membrane of the intestine is anæmic
and of a dirty grayish appearance ;   that of stomach shows no
changes apparent to the naked eye.   There is no solid fœcal
matter in intestines, but both the large and small intestine
contain a small amount of a chalky-colored mixture of mucus
and liquid fæces.   Several Peyer's patches near the ileo-
cœcal valve present the shaven-beard appearance very slightly.
Spleen is strongly fixed in the hollow of the diaphragm by old
adhesions, is enlarged by one half, is soft, and of a dark red-
dish-brown appearance.   The kidneys are about normal in size,
markings indistinct in places, capsules removed with some dif-
ficulty, tearing the cortex as it comes away.   Uterus is nor-
mal in size ;   four small cysts in left broad ligament ; a cyst,
size of large bean, and containing a clear serous fluid, is found
near the centre of right ovary.   Mucous membrane at fundus

is swollen and congested. No recently ruptured Graafian fol-
licles to account for uterine hemorrhage on last day of life. Os
is slightly eroded.

The liver is collapsed and partly covered by the small in-
testines ; it weighs thirty-eight ounces, is soft, flabby, surface
much puckered, and margin very sharp.

The dimensions of the organ are diminished in every direction,
the left lobe being especially thin.

The gall bladder contains about an ounce of thick dark bile,
and its communication with the intestine is unobstructed. The
tissue of the liver is flabby and moist, and, except a part of
the right lobe, is almost bloodless ; the left lobe is of almost a
uniform ochre-yellow color, but even here the vessels surround-
ing the lobules can be seen to be slightly injected, while the
centre of the lobule is of a yellow or grayish-yellow color ; near
the thin margin of this lobe the capillary injection entirely dis-
appears. In the right lobe the process is not so far advanced ;
this lobe, in its thickest portions, is congested at places, and
near the centre of this lobe are several moderately large ex-
travasations of blood into the liver tissue ; these spots are so soft
as to scarcely bear the most careful handling, and here the
tissues seem completely disorganized.

EDITORIAL DEPARTMENT.

THE BATTEY-TAIT OPERATION.

THERE is no subject which has recently come up in medicine that is more interesting or important, than the question of the legitimacy and scope of the operation for the removal of the ovaries. It is now twelve years since Prof. Hegar in Germany, Mr. Lawson Tait in England, and Dr. Robert Battey in the United States—almost simultaneously each did his first operation. Dr. Battey was the first to publish his case (which was also the first successful one) under the title of "Normal Ovariotomy," a somewhat unfortunate name, which was soon abandoned for the one suggested by the late Dr. J. Marion Sims of "Battey's Operation."

Various other titles have been suggested for the proceeding, as "Castration of Women," "Spaying," "Oöphorectomy," "Removal of the Uterine Appendages," "Ovariotomy." This diversity of titles is fairly indicative of the diversity of principles underlying the various operations. The term ovariotomy would probably be the most acceptable, if it were not that by general usage it has come to be restricted to the removal of tumors of the ovary.

Such being the case I shall not attempt to add another to the too numerous titles already used, but shall briefly consider the various modifications of the procedure as the Battey-Tait operation. At the meeting of the International Medical Congress in London, Dr. Battey reported a table of nearly two hundred completed operations which had been performed up to May, 1880.

It is certainly not extravagant to estimate the published cases since then, up to date, as from two to three times that number. One cannot help being struck with the fact, that some great operators are conspicuous by their absence from the published lists. Thus where Mr. Tait does a hundred operations, Spencer Wells only does one or two. Yet these two distinguished men must see about the same class of cases, indeed we know that in some instances they see the same individual patients, for Mr. Tait takes particular delight, apparently, in telling us that in many of his cases an operation had been previously refused in London. I think Dr. Keith has only done the Battey-Tait operation once or twice.

These facts speak volumes ; they may be considered as fairly indicative of the estimation the operation is held in by different competent observers.

It would seem that we might now be in a position to define the indications for the operation, and to settle the question of its legitimacy and scope. But such is very far from being the case, and it is certainly remarkable that a capital operation has been done so many times within such a short period, on such vague grounds and with such wild hopes. There has been no dearth of opposition to the operation, but it has been chiefly active, either in the form of shrieks about the iniquity of spaying women, or in impugning the motives and veracity of Mr. Lawson Tait. On the other hand, the advocates of the operation have manifested a certain fervor of statement and belief much more suggestive of a religious revival than a scientific discussion.

The operation has been hailed as a cure-all for the woes of womankind, and the more desperate the case with the more enthusiasm has its performance been urged. Again, numbers of cases have been reported cured, to which grave exception must be taken.

There has been far too little expression of opinion on the subject from the profession at large. Perhaps it is too soon to expect that impartial summing up of the reasons for and against, which has to come from outsiders, not from operators simply, before the operation takes its due rank among the settled pro-

cedures in surgery. But a few more discussions such as that held
at the Am. Gyn. Soc. meeting in 1880, would do more toward such
a settlement of the questions at issue, than scores of cases as now
published.

The necessity of having-well defined grounds for operating is
all the more pronounced because of the great dangers inherent in
the operation to life and happiness, and the fact that it is usually
done in the comparatively young. Thus the average age in Dr.
Battey's table, calculated from 132 patients, is under 32 years.
Of the actual dangers to life far too little has been said. Mr.
Lawson Tait in especial has been singularly rash in minimizing
the risks, and he has probably done a great deal of harm in
encouraging men to undertake a so-called "easy operation,"
which too often has ended fatally for the patient. Mr. Tait's
own cases, and those of Mr. Savage, are marvellous, and they go
far to justify them in personally operating—but the same does not
obtain among their followers. I think, moreover, we shall find
later on, that the greater the need and justification there is for the
operation, the more dangerous it is apt to be. In Dr. Battey's
table the mortality is eighteen per cent., and I do not think from
a tabulation I have made of the cases since then that the general
average has been lowered.

Nor is the question of mortality the only one we have
to take into consideration—for turning again to Dr.
Battey's table, and taking all the cases in which the ultimate
results and which recovered are given, we find that 65% are put
down as "cured," while 22% were "benefited," and 11% were
"not benefited," the remainder not stated. In making up our
minds as to the dangers and benefits of the operation, we must
not forget that a certain number who recover are not cured.
Taking Dr. Battey's own very fair analysis of the results as a
basis, we may conclude that of ten women who are operated on
for all the causes that the Battey-Tait operation has been done
to relieve, he claims that five are "cured," two are "benefited,"
while of the remaining three one recovers, but "not benefited,"
while the other two die.

Mr. Lawson Tait's claims for the operation, based mainly on his own experience, are very much more sanguine.

I think it will be hardly questioned that, taken right through, the average Battey-Tait operation will be found more difficult to perform, and more dangerous to life, than the average ovariotomy. Such at least is the opinion of such men as Spencer Wells and Dr. Keith. The method of operating which was for some time under dispute, may be now considered as settled. In Dr. Battey's first ten cases, which were largely tentative and experimental, he used the vaginal method, but it is now conceded by him, that in many cases it is wholly inapplicable, and it certainly has never given the same brilliant series of results that the abdominal incision has in Mr. Tait's hands. Several other modifications of the operation introduced by Mr. Tait are of great importance. One of these is his peculiar manner of ligating the pedicle by means of the " Staffordshire knot." I am quite sure that this most valuable improvement is by no means properly understood, when in the last edition of his book Spencer Wells makes no mention of it, and one of our own most distinguished surgeons ties an entirely different ligature and then blames " Tait's knot " for not controlling the bleeding. Another most important addition by Mr. Tait is his removal of the Fallopian tubes. I can hardly share his belief in the great importance of these organs in health—a point which he asserts with great positiveness, but on which he adduces no evidence,—but there can be no question of the great mischief they may produce when diseased. More especially in the condition of pyo-salpinx their presence is fraught with great danger. In cases of hydro-salpinx, however, I question the justifiability of resorting to their removal, or the extreme difficulty or inutility of tapping, as he asserts. Another procedure which Mr. Tait has resorted to, brings up an even more serious question :— he has in certain cases removed the tubes and left the ovaries, which were healthy, behind. It would seem as if he thereby rendered the women liable to recurrent attacks of pelvic hematocele and peritonitis, a difficulty by no means met by his mere statement that he believes that the vast majority of ripe ova fall into

the peritoneal cavity any way, without producing any mischief. The only other surgeon I know of that has similarly removed the oviducts, leaving the glands behind, is Mr. Wood, of Cincinnati. He did so (Am. Gyn. Trans., 1880) on the declared ground that he thought the woman would be happier, would derive more sexual pleasure than if they were removed. And this brings us to the questions : What are the effects of removal of ovaries and tubes, 1st, on the sexual system ; 2d, on the general system ? On the answer to these questions will depend to a considerable extent the decision as to the morality of the operation, but on these points the evidence is singularly defective.

It has been pointed out very ingeniously by Dr. Battey, that certain of the discrepancies are to be accounted for by the fact that a portion of the ovarian glandular tissue has been left behind, and so has kept up for a time at least a remnant of functional activity.

He instances a case of his own in which this explanation held good, and there is one case, I believe, on record in which pregnancy occurred subsequent to a so-called Battey's operation. But excluding the rare cases in which a third ovary is present, and taking only those in which the ovaries have been completely removed, what are the effects on the sexual system ? The first and most important result is of course the absolute arrest of ovulation, and therefore the possibility of child-bearing, and on this all are agreed. Next comes the question, What is the effect on menstruation—is this at once and completely arrested ? The answers are not unanimous, because many cases have been reported of the continuance for a longer or shorter period of a monthly flow. It is to account for these anomalous cases that Dr. Battey suggests a remnant of ovarian glandular tissue or a third ovary, while Mr. Tait boldly alleges that continued menstruation is due to non-removal of the tubes, and asserts that if these are completely removed, the function is at once definitely arrested. I must confess I do not think either explanation is entirely satisfactory, for I recall at least one case in which all the ovarian tissues were removed with the tubes close up to their emergence from the uterus, in

which such monthly flow reappeared. It seems more physiological to suppose that the habit impressed by the ovarian stimulus (ovulation) on the nervous system, leading to menstruation, may to a certain extent remain and only gradually die out. A formed habit may continue its periodical effects, though the present cause is removed, and in consonance with this view we should expect to find that the more completely this habit has been formed, the more perfected and ingrafted on the woman's system the functions of ovulation and menstruation have been, the more likely would such after-menstruation be to continue. In other words, the woman of thirty who has borne children, and becomes the subject of the Battey-Tait operation, will be more likely to continue the after-effort at menstruation than her virgin sister of eighteen who has been similarly treated. On the structure of the uterus the effect would seem to be to bring about the same diminution in size which follows the physiological change of life. But to this there may be exceptions to be noted below.

The question of the influence of this operation on the sexual appetite is one that has been eagerly canvassed, and is one of importance and delicacy. From the nature of the subject the question is one on which the evidence is deficient. It may be at once readily granted that the operation will restore a certain number of women to the possibility of fulfilling their marital functions by removing a source of pain—such as an inflamed and prolapsed ovary which had been a bar to connection before. But the question as to the effect on the woman herself is quite undecided. Dr. Thomas has reported a well-known case, in which the woman, passive in her sexual relations before the removal of both ovaries, became aggressive afterward. The question becomes an important practical one in those cases where it is proposed to do the Battey-Tait operation for nymphomania or incorrigible masturbation. Such operations have been done, although Dr. Battey is on record as strongly opposed to them, but the results obtained have not been satisfactory. The time element in this relation seems important, and its consideration unduly neglected, and the remarks already made with regard to the question of after menstru-

ation as dependent on formed habit, apply here to a still greater extent. Judging from the analogy offered in men in the case of eunuchs, the removal of the testicles has not proved sufficient to arrest sexual desire, and consequently in the East the penis is also amputated. It would be natural to expect that while the Battey-Tait operation might prove curative in very recent cases of masturbation, it would not in chronic cases or in nymphomania.

Turning to the ordinary cases for which the operation is done, —not for these loathsome moral diseases,—it is probable that sexual desire is not destroyed at once, though possibly, as a rule, it is diminished, and undergoes a somewhat premature decay. To settle this point we need observations extending over a longer time after the operation than most of the cases so far published.

Turning to the effects the operation has on the general system, we are still more wanting in evidence. Here the analogy with the male is not apt to hold so good, because in the man the alterative influence of puberty is much greater, in its outward manifestations at least, than in the woman. Such is the case, for example, with the voice—for the girl's and the woman's voices are at least an octave nearer together than the boy's and the man's. Similarly, the appearance of hair is much more marked in the male than the female. All of this would lead us to infer that the general change following the Battey-Tait operation would not appear as great as that following castration in man. The changes that do occur are less in the nature of a reversion to the type of girlish childhood than in the approximation to a masculine type. The closest analogy is to be found in cases of atrophy or non-development of the ovaries— Nature's Battey-Tait operation. In these we find the absence of ovulation and menstruation, the tendency to hair-growth on the face, and the excessive development of adipose tissue ; but neither in the voice nor in the contour of the form are there any indications to show the complete abeyance of ovarian function. The chapter in physiology that this question opens up is a most interesting one, and cases carefully observed after the Battey-Tait operation for several years, will furnish the data that will enable us to fill in its at present meagre and blank pages.

If, now, we turn to an analysis of the various published cases of the Battey-Tait operation, we find that they can be arranged under five different heads, according to the indications for which the operations were done. It has been proposed to remove the ovaries (as suggested by Hegar and Battey), or the ovaries and tubes (as advocated by Tait) :

I. For the arrest of the menstrual molimen where this is inflicting grave injury on the patient because unrelieved by monthly flow, due to :

(*a*) Congenital absence, or quite rudimentary development, of uterus and vagina.

(*b*) Traumatism leading to obliteration of the utero-vaginal canal.

II. For the relief of organic disease, otherwise incurable, of the ovaries.

III. For the relief of certain nervous symptoms—especially pain—the ovaries themselves presenting no special or recognizable evidence of organic disease.

IV. For the arrest of : (*a*) uterine hemorrhage, and (*b*) the growth of uterine tumors.

V. For the cure of certain uterine diseases and salpingitis.

There can be little question that, in the cases which truly belong to the first class, the operation is entirely justifiable. Fortunately, they are not numerous, but Dr. Battey cites two,— one belonging to each subdivision.

In the congenital cases, the best proof of the necessity for operative interference is found in the fact of the hopelessness of all other modes of treatment. Most of the older text-books allude to these cases merely, and pass them over with the remark that nothing can be done for them. The only suggestion that has proved of any efficacy—and that not of very much—was the plan proposed by Dr. Thomas. In view of the occasional occurrence of vicarious menstruation, he suggested bleeding from the arm at each regular monthly period. If menstruation consisted simply in the loss of so many drachms and ounces of blood, this proceeding would, no doubt, meet every requirement, and its comparative

safety would, to a certain degree, compensate for the inconvenience of systematically carrying it into execution.

But the phenomena, local and constitutional, of menstruation, are very much more complex than that; and where grave mischief was taking place, as in Dr. Battey's case, the radical operation would offer the only cure, or even hope of benefit.

In the second set of cases, in which the perviousness of the utero-vaginal canal is obliterated by some traumatism—usually due to parturition—the first question to be setttled is the possibility of restoring the channel itself by an operation.

This may be an extremely difficult matter to decide, but unless it is fairly evident that the obstructing cicatrix is more in the nature of a circular membrane of not too great depth, and not a long impermeable band, reaching high up and involving the utero-vaginal juncture and the peritoneal cul-de-sac, it would seem far preferable to take the risk of the Battey-Tait operation, which is a known quantity, to the unknown dangers of an elaborate dissection. The mortality from the operation in this class of cases should be very low, as the seat of malformation lies not in the ovaries but the uterus. Several cases of the traumatic class where the operation would have been applicable have been published, and it has been done successfully. I know of one case in this city in which it would have been in order, where the reckless use of the forceps before dilatation of the os was complete, led to complete circular rupture of the uterine neck and vagina. Some weeks subsequently—for the patient survived this extraordinary accident—on making a vaginal examination a closed canal was found, the uterus being unappreciable by the finger.

In these cases, therefore, both of the congenital and traumatic forms, Dr. Battey has done a good thing in pointing out their only method of cure.

II. Cases of organic disease, otherwise incurable, of the ovaries.

This class, I think, comprises most of the patients in whom it is justifiable to do this operation. Given a patient with advanced ovarian disease, for the relief of which all other rational methods of treatment have been faithfully tried and in vain ; whose health

is being undermined, and her happiness and usefulness in the community destroyed, then the Battey-Tait operation is entirely justifiable. This is not the time or place to consider all the forms of disease of the ovary that come under this class, or the diagnostic signs by which we recognize them, but there are one or two special points worth considering, as they have not generally received their due share of attention. The first of these is the essentialness of objective signs and not merely subjective symptoms in establishing a diagnosis. It may sound like a truism to make a bald statement like that, but more mistakes are made, more unjustifiable operations undertaken, and more lives sacrificed because this essential point is overlooked, than can be estimated. We have to be continually on our guard against the deceptions, unintentional often, of the patient's feelings. Take, for instance the symptom of pain. In nine cases out of ten, a woman will refer *any* abdominal pain to the ovaries. Now if the glands are healthy, we will find, on bimanual pressure, that the sensation produced is *not* painful, but merely the characteristic, distressed, sickish feeling. As a matter of fact the ovaries are remarkably insensitive in health, and consequently exquisite tenderness and shrinking on touch or pressure, excluding the hysterical element, is a most valuable sign in disease.

The importance of displacement, prolapsus of the ovaries, has not received the recognition it deserves. The anterior displacement is rare, but where it is producing serious mischief the Battey-Tait operation offers the only rational hope of cure.

The posterior displacement, in various degrees of severity, is very much more common, but, fortunately, it is not generally so unamenable to treatment.

Unless the prolapsus is of an extreme degree, we may hope to do much by mechanical means. Where, however, an ovary lies so low down as to be the first thing encountered by the finger after entering the vagina, especially if it is bound down by adhesions or by a heavy and retroverted uterus, the chances of any thing short of the radical operation effecting a cure, are very slight. It is remarkable how quickly an ovary misplaced to this degree will

undergo pathological changes. It is probable that it was in a
state of acute or chronic congestion in the first place, either giving
rise to or accompanying the displacement. These ovaries so soon
become the seat of cystic degeneration, that probably this patho-
logical lesion must be, in large part, due to perverted functional
activity. The extent to which adhesions are present in these
cases of ovarian disease is a very important question. I believe
it is, broadly speaking, true, that the more extensively the ovaries
are matted up in the products of an old peritonitis, the greater
will be the necessity for the operation. This suggests the unpleas-
ant reflection, that the greater danger is there of it being fatal If
it should ever become recognized as sound practice to follow the
example of Mr. Lawson Tait to make the abdominal incision and
drain cases of pelvic abcess and peritonitis, it is probable that we
should save many patients from the necessity subsequently of hav-
ing their ovaries removed.

A large number of cases of ovarian disease are undoubtedly the
secondary results of effusion of the products of inflammation into
the pelvis, in the meshes of which the glands become entangled
and subsequently diseased. It is becoming more and more recog-
nized as sound practice that in cases of pus in the pleural cavity,
drainage is the best treatment alike for the acute febrile condition
and the avoidance of future pulmonary disease consequent on col-
lapse of the lung and extensive pleuritic adhesions. It is by no
means impossible that it may yet prove feasible to carefully lib-
erate the ovaries from bands of adhesion, and then get rid of the
products of inflammation by careful drainage.

In any event the line of thought Mr. Tait has opened up by his
daring performances is a most suggestive one. And in the treat-
ment of otherwise incurable ovarian disease by removal of the
organs, he has been as sound in principle as he has been success-
ful in practice.

III. The third class of cases, for the cure of which the Battey-
Tait operation has been proposed and has been done, embraces
the nervous ones. And here it may be well to again emphasize,
that by this class is not meant those cases in which nervous symp-

toms play an important part simply, for that would practically in-
clude all five divisions. But I mean those cases in which the
nervous disturbances constitute *the* disease ; in which the opera-
tion is done for their relief, and not for a known diseased con-
dition of the ovaries themselves.

It is about this class of cases that the hardest fight has raged
between the opponents and the advocates of the procedure. And
it must be at once acknowledged that it is in this class of cases
that the greatest room for doubt arises. When a man removes an
ovary which has undergone marked pathological change, and yet
fails to relieve his patient, his after-thoughts are no doubt bitter
and sad ; but he has at least the satisfaction of knowing that he
has removed an organ which could not be of any further service
to its possessor, and might be a future source of danger. But no
such consolation awaits him who removes what are apparently
healthy glands, and the patient's sufferings are not abated. If he
is honest, he must know that he has inflicted a positive harm on a
fellow-being, and he may well ask himself if he was justified in the
process of reasoning which has led to so deplorable a result.

It may well make one pause in accepting such a responsibility
where the conclusion is so largely based—not on physical signs—
but on more or less rational inferences. The whole subject of
reflex influence is so obscure in its physiology that it is simply
with a feeling of amazement that one reads on what mere guesses
this most hazardous operation is undertaken. In this branch of
the subject we should expect that the neurologists' aid would be
valuable, yet in how many cases in which the ovaries are removed
for " pain " or " epilepsy " or " insanity," is their counsel sought ?
In these cases accuracy of diagnosis is pre-eminently needed, to
justify the morality of the proceeding. These remarks are neither
gratuitous nor uncalled for ; case after case could be cited in which
the operation has been undertaken on the most inaccurate diag-
nostic grounds. It is at all times a dangerous proceeding to oper-
ate for the relief of reflex symptoms, which are in large part
entirely subjective ; but if it is ever done, one would think the
first point to be settled is the credibility of the witness. Now it

is quite notorious how largely the hysterical element enters into many of these candidates for the Battey-Tait operation.

I confess I cannot see how that element can be satisfactorily excluded in patients sent to some noted operator from a distance, except by prolonged and minute personal observation. It may be said that the doctor who sent the case to the surgeon had the patient for a long time under observation. Grant it, and in return I would like to ask what man is justified in doing a capital operation like this on another man's diagnosis? Many of the cases thus hastily operated on, are as hastily reported cured, quite honestly, I do not doubt, but they are singularly fallacious. It is well known that profound impression made on the nervous system by such a grave operation will often temporarily modify and arrest the most deep-seated morbid processes, like epilepsy, and yet the effect is *not* permanent. But the sanguine operator is quite sure the patient is cured, for was she not quite well when he dismissed her?

In considering the general question of the morality of the operation, I drew attention to the fact that most of the subjects of it are comparatively young. That fact, and the civil condition of the patient, have an important application with reference to these nervous cases. A certain number are unmarried women, while others, the married, are unhappily so, as regards their sexual relations. In these cases the extent to which their symptoms are due to unsatisfied sexual needs has to be carefully considered. If they were condemned like the anchorites of the desert to a perpetual celibacy, it would be, no doubt, eminently well worth considering whether the removal of their reproductive glands was not in order. But such is not the case in the great majority of cases, and it may be safely affirmed that, where one may be relieved by, if she survive, the Battey-Tait operation, at least twenty will be cured by a congenial marriage and the bearing of children. There is another important element in the consideration of this class, and that is, that so many of them are addicted to the abuse of stimulants and narcotics. Mr. Lawson Tait has borne testimony to the harm many of his countrywomen are doing themselves by their abuse of alcohol to relieve pelvic pain, and

he seems quite unconscious of how much that admission may in-
validate their testimony when they come to see him to have their
ovaries removed. Dr. Matthews Duncan stated it as his opinion
that the majority of this class were addicted to the habitual or
periodic use of opium. And who can estimate, in addition, how
much the bromides, chloral, patent nostrums, "vegetable mix-
tures," "elixirs," and the nameless compounds which are so ac-
cessible now to women, have to do not only in the perpetuation, but
also in the creation, of many of these symptoms? Turning now
to the individual symptoms, for the relief of which the operation
has been proposed, they may be grouped under four sub-
divisions:

(*a*) Pain.

(*b*) Epilepsy and hystero-epilepsy.

(*c*) Insanity.

(*d*) Perversion of the sexual instinct.

(*a*) "Pain referred to the ovary," is a very frequent diagnosis
in the literature of the subject, but if it is more closely examined
it almost invariably should be, "pain referred to the pelvis." It is
hard to see how a perfectly unmistakable diagnosis of neuralgia
of the ovary can be made. Even supposing the hysterical ele-
ment and the use of narcotics can be excluded, and the honesty
of the witness is beyond cavil, how are we to be quite sure of our
diagnosis? If it is unilateral pain, are we to remove one ovary
and leave the other behind? That has been tried, but not with
brilliant success. If the pain is worse at the menstrual epoch,
what pain, pelvic or other, in the woman's body is not worse at
that period? If we are to be logical, why should we not remove
the ovaries in those women who suffer from sick headache (mi-
graine) every month? A far closer analogy can be traced between
such megrim and menstruation than between it and ordinary
ovarian neuralgia. In the case of megrim, we are supposing, the
sick headache only appears at the monthly period, while the
ovarian pain is present at other times and only suffers an exacer-
bation then. But granting the accuracy of the diagnosis, that
you can be as sure of a neuralgia being located in the ovarian

nerve as you can be of one in the second division of the trigeminus, what are your chances of curing your patient by removing the peripheral expansion of your nerve? This question may, perhaps, be best answered by asking in how many neuralgias elsewhere it is deemed right to exsect a nerve? Has it not been tried time and time again, for instance, for tic-douleureux without relieving the patient? Is it not true that, even where a neuralgia starts as a purely local irritation, it leads to structural changes in the related nerve-centres, which, once established, are quite out of reach of the scalpel? Long before it would be deemed justifiable by the most radical gynæcologists to remove the ovaries, such a central nervous impression has been made.

The argument that has been advanced, that the nerve-centre, like the nerve fibre, will undergo secondary degeneration, and so gradually end it, is quite too theoretical to need discussion. How many neuralgias, however, are such purely local affairs? I think it is true that the vast majority of true neuralgias, not mere temporary discomforts,—but acute pain which comes and comes again in a particular nerve or nerves,—are the local expression of a constitutional condition. It is only on this pathology, that neuralgia is the expression of nervous malnutrition, that the immense influence of heredity in its causation can be accounted for. If it is a purely peripheral irritation, how is it that we find it in families alternating with epilepsy and even phthisis? No doubt the local expression, the localization of the pain in a particular nerve trunk, is due to some weakness or irritation at the periphery in many cases, but in a healthy condition of the central nervous system such an irritation would never become a neuralgia.

The results of treatment corroborate this pathology, for the various remedies of value in neuralgia—counter-irritation, galvanism, nerve tonics as phosphorus—all act by improving the nutrition of the nervous structures.

The instance of the irritable and painful stump, which is cured by section of the offending nerve fibres, so often cited in justification of the Battey-Tait operation done for pain, is quite beside the

mark, and entirely overlooks the distinction between an injury and a disease.

This class of cases are those particularly apt to be benefited by the systematic treatment formulated by Weir Mitchell, and this plan bears a curious inverse relation in value to the Battey-Tait procedure. While the Weir-Mitchell system is contra-indicated in the inflammatory and organic type of ovarian disease, it is of great value in the neuralgic and functional perversions, and the exact reverse, I think, holds good of the Battey-Tait operation. If the above views are correct, as I believe them to be, we must unhesitatingly conclude that removal of the ovaries for the relief of pain simply, for the cure of neuralgia is, entirely unjustifiable.

(*b*) Epilepsy and hystero-epilepsy.

It is no new thing—the proposal to cure epilepsy by an operation. Some time ago, but not so long that we can afford to be too contemptuous in speaking of it—our medical forefathers trephined the skull to allow the devils to escape. Perhaps our descendants may in turn be inclined to smile, that in our generation it should be seriously advised to remove the ovaries for the same disease. It is true we do not any longer talk of devils in this connection, but it is a question whether many of the so-called reflex symptoms are not as much unknown quantities.

Since Brown-Séquard's researches on epilepsy, comparatively few operations for its cure have been proposed, except when a well-marked aura was present. But they have not been such as to warrant much belief in their value, even in the simplest cases, where the aura travelled on easily ascertained paths, as along the nerves of the arm to the hand—how much more chimerical is the hope of benefit when the aura is referred to an organ like the ovary, under the influence of a most complex nervous supply from both the cerebro-spinal and sympathetic systems. We know so little of the essential nature of epilepsy, that at most the advocates of the value of the Battey-Tait operation for its cure would have to base their claims on clinical evidence. And if we examine the published cases, there is not a single one on record of long-enough duration to warrant it being pronounced a cure. No

better witness to the truth of this assertion can be found than Mr. Tait himself, and this is what he says about a case that is ordinarily put down as a " cure " : " The second case I operated upon was a girl, aged eighteen, who had been imbecile from birth, and who had developed the most violent menstrual epilepsy from the time of the molimen. * * * I operated on May 9, 1880, with the result of completely arresting menstruation and *abolishing the epilepsy*. * * * She gives slight indications of an increased noisiness and loquacity at the time at which her periods should occur and occasionally at these times she has an attack of *petit mal*.

The words I have italicized are significant, and it is certainly an extraordinary conception of nervous pathology which can allow of the statement that the epilepsy is abolished while the attacks of petit mal continue !

Another case I have heard of in which the ovaries were removed for epilepsy with an aura said to be ovarian, but which turned out to be, after the operation, from the epigastric region ! The Battey-Tait operation for the cure of epilepsy, then, must be absolutely condemned. In hystero-epilepsy the question is more undecided. I imagine that the less marked the epileptic element is, and the more pronounced the hysteroid, of this vague disease, the more is to be hoped from the operation. It has occurred to me that there is a point which might aid us very materially in deciding as to the probable benefit from the operation. Some years ago M. Charcot drew attention to the fact that in certain hystero-epileptics the fits could be arrested or modified by strong pressure over the ovaries. Now in certain cases this does not obtain, and no amount of pressure exerts any appreciable influence, but in those in whom it *does*, it is likely at least that the Battey-Tait operation would effect permanently what pressure did temporarily.

This whole subject is so vague and ill-defined that the justifiability of the operation is, as yet, not established.

(*c*) Insanity.—Several cases are on record in which removal of the ovaries was followed by improved mental capacity, but the statements are vague.

On the other hand, Dr. Thomas has had two cases (quoted by Dr. Pallen, Int. Med. Congress), in which the development of insanity followed the removal of the ovaries.

A condition that one might *a priori* think to be peculiarly likely to be benefited by the Battey-Tait operation, is menstrual mania, but the following case shows that, except as a last resort, it is entirely unjustifiable. Dr. Fordyce Barker related a case (Am. Gyn. Soc., 1880,) which is so important that I will quote a portion of it : "At the occurrence of each menstrual period she had acute mania for two or three days, and in the intervals between her menstrual periods, she was morbid in her tastes, her feelings, and her sympathies, and she was in every way a source of great misery to herself and unhappiness to her family. I placed the patient upon bromide of potash three or four days previous to the occurrence of the menstrual period and during menstruation. As soon as the symptoms of acute mania manifested themselves, I directed that she should receive one twentieth of a grain of hyoscyamia, to be repeated every two hours until she became quiet. Subsequently the quantity of hyoscyamia was one sixth of a grain three times a day. It has now been nearly eighteen months since this plan of treatment was adopted, and menstruation has become much easier than formerly, and the change in her moral and mental condition has been most remarkable. By the use of hyoscyamia during menstruation she becomes perfectly quiet and tranquil. I have pushed the drug to the production of its constitutional effects in some degree, and the result has been most happy, but generally not more than three or four doses are required. Her morbid likes and dislikes have all subsided, and the melancholia which existed in the intervals between menstruation has entirely disappeared. She still continues to take hyoscyamia at the time of menstruation, now only one tenth of a grain three times a day."

I think this case contains an important warning to those who so eagerly resort to this operation. I do not think that Mr. Tait would have hesitated to operate on this patient, and the like is perhaps true of Dr. Battey ; and yet

even had the operation terminated in the recovery and cure of the patient, I should say it was entirely unjustifiable, for she was practically cured by Dr. Barker, without subjecting her to the terrible risks of a fatal result. I do not know of any attempt to formulate any principle with regard to these cases of insanity and the probable effect of removal of the ovaries on their mental condition. But I think some conclusions reached by Dr. Skene some years ago, in the course of extended study at the King's Co. Insane Asylum, on the relations between gynecology and insanity, are applicable here (see ARCHIVES, Feb., 1880) :

" Well-developed insanity with impaired general nutrition causes suppression of the functions of the sexual organs. The influence of insanity on this class of diseases (functional diseases) is most favorable. It may be stated fairly that such diseases disappear on the occurrence of mental alienation. Organic diseases of the sexual system exercise a most important influence in causing insanity, and tend to retard recovery from it. A careful consideration of this subject has led to the conclusion that *acute* affections of the brain and nervous system, that are wholly due originally to disease of the sexual organs, will be relieved in a large majority of cases by curing the primary affection. The effects of treatment of the disease of the sexual organs will be in proportion to the duration and severity of the mental derangement. In subacute mania, caused or aggravated by disease of the sexual organs, marked benefit or prompt recovery may be expected to follow the cure of pelvic disease. On the other hand, chronic mania, associated with the disease of the sexual organs, will often remain unchanged after the local disease has been relieved."

In applying these general principles to the particular question of the value of the Battey-Tait operation in the relief of insanity, we are forced to the conclusion that it depends mainly on the condition of the ovaries.

If these are affected merely functionally, the operation is wholly out of the question. If, on the other hand, there is evidence of structural disease—organic changes in the ovary which are leading to mental impairment,—then the operation is entirely justifi-

able, and the more promptly it is done the more likely it is to prove curative.

(*d*) But little need be added here to the remarks already made as to the non-justifiability of the operation for cases of perversion of the sexual instinct—self-pollution and nymphomania. For the first of these conditions, if the bad habit is not too strongly formed, if the operation is done early enough, it might prove of benefit, especially if clitoridectomy was done at the same time. But where the habit is fully formed,—where it has led to that peculiarly intractable form of insanity due to masturbation,—it is not likely that the operation would be of the least benefit. It is probable that only in the advanced cases most men would feel justified in advising, or be permitted to do, an operation, and therefore the value of this field for the operation, or the prospect of its being enlarged, is not promising. With regard to nymphomania, such evidence as there is is quite against the justifiability of the operation.

IV. The fourth class of cases in which the Battey-Tait operation has been eagerly championed comprises those in which life is endangered by (*a*) profuse uterine hemorrhage or (*b*) the growth of uterine tumors. Very often the second of these, the presence of large tumors, is mainly dangerous by giving rise to the first, the hemorrhage. But even where there is no tumor present, but where there is profuse uterine bleeding, either at the monthly period or between times, the Battey-Tait operation has been done with the hope of arresting it. It has been argued on two grounds that this would prove curative : the first one, because you would "cut off a portion of the blood-supply of the uterus"; the second one, that "the effect on the nervous system would be such as to bring about the change of life and the consequent diminution in the size, etc., etc., of the uterus."

The first of these arguments needs only a brief consideration. The uterus is mainly supplied by the utero-ovarian arteries, branches of the internal iliacs. After supplying the uterus, they anastomose freely with the branches of the ovarian arteries derived from the aorta. Now, while the ovarian arteries would be

tied in the pedicle after a Battey-Tait operation, and so cut off
this source of supply to the uterus, the utero-ovarian (anasto-
mosing and ovarian branches) would also be tied, and the whole
supply from the internal iliacs would be concentrated on the
uterus. If, therefore, the soundness of the procedure rested on
the circulatory changes it was supposed to produce, it would be
entirely irrational. The second argument, the diminution of the
ovarian nervous influence, is more plausible. But the question is
by no means so simple as it looks, and its determination involves
the further one as to what is the cause of the hemorrhage. It
may be well to recall a fact connected with Dr. Battey's first
operation, namely, that it was followed by metrostaxis, from
which the patient had been quite free before. That serves to
show that we must make some distinctions between cases.
Again, Dr. Trenholme reported a well-known case of alarming
recurrent hemorrhage, due to a myoma, as cured by the Battey-
Tait operation, and yet, some months later, Dr. Byford stated at
the Am. Gyn. Society that this very case was then under his care,
the bleeding having recurred as profusely as before the operation.
If we analyze the causes of hemorrhage in these cases, we find
them to be of four kinds :

1st.—A diseased condition of the uterine mucous membrane.

2d.—A certain nervous influence reflected from diseased
ovaries, leading to too frequent or profuse menstruation.

3d.—The hemorrhagic diathesis.

4th.—The presence of neoplasms.

If the hemorrhage is due to a diseased condition of the mucous
membrane of the uterus it would be quite unphysiological to ex-
pect to cure it by this operation. Even though the menses should
be promptly arrested—of which we cannot be sure—there is
no evidence whatever that the diseased process would be ; on the
contrary, there is a possibility that it might take on a malignant
character. If the hemorrhage is secondary and due to a morbid
ovarian influence, then the question resolves itself into one as
to the justifiability of operating for the ovarian disease. If it is
decided that the ovaries are incurably diseased, and that they

come under the second class of our whole subject in whom the operation is justifiable on the grounds given above, then the operation would also be likely to cure the uterine symptoms. But even then it is by no means certain, and it would not be right to operate for the mere relief of a symptom—however serious. In the cases where the excessive menstruation is due to the hemorrhagic diathesis it is evident that the less coagulability the blood has—the more dangerous the Battey-Tait operation itself becomes, and the more the risk of secondary hemorrhage. It therefore resolves itself into the question, is the danger of the patient's dying before the menopause from the capillaries of the uterus greater than the danger of sudden death from secondary bleeding from the ligated vessels in abdominal cavity and walls *plus* the other dangers of the operation. Put in that way we think few would care to take the responsibility of the operation, but would rather trust to the ordinary and safer methods, such as the tampon, the use of astringents, and styptics, and, if need be, the cautery. When the hemorrhage is due to the growth of neoplasms—the possibility of checking it is bound up with the question of the arrest of their growth by the Battey-Tait operation. It is not out of place to remark here how little is said by our English brethren of removing the offending growths by the vagina. In a paper by Mr. Knowlsey Thornton (Am. Gyn. Soc., 1882), he considered "The Relative Value of Hysterectomy and Complete Removal of the Uterine Appendages for the Cure of Fibroids of the Uterus," as if these were the only alternatives in all cases. Dr. Thomas pointed out in the discussion that in very many cases the tumors could be removed by the vagina. Before the question of the Battey-Tait operation should even be considered, the possibility of their removal by the vagina should be absolutely excluded. If the case is such that from the situation of the tumor, or, from its size, its vaginal removal is plainly impossible, it by no means follows that the other is in order. The statement is often enough made that the removal of the uterine appendages will arrest the growth of these tumors,—a number of cures have been reported,—but I think on insufficient evi-

dence. There are several facts that stand much in the way of accepting this belief, and one of them is that many of these neoplasms have their most luxuriant growth after the menopause. Such is notoriously the case with cancer of the uterus ; and is it not a fact, settled beyond dispute, that it first begins to show itself coincidently with a cessation or diminution of ovarian activity ? I was very much struck, in reading recently the report of Dr. Thomas Keith's cases in the Edinburgh Royal Infirmary (reported by his son, Skene Keith), with the remarkable series of ten successful hysterectomies. In two of these cases the uterine tumor first gave evidence of its presence at or after the menopause, while the average age of all ten patients was thirty-six years. Similarly, Dr. Drysdale has recorded it as his experience that these affections were apt to develop more rapidly after the menopause (Am. Gyn. Soc., 1880).

Furthermore, it has been stated by competent observers that fibroids of the uterus have been known to disappear during pregnancy. The growth has been not only arrested, but the already formed tumor has undergone absorption. In other words, the highest functional activity of the organ sometimes has brought about more than what is claimed for the exactly opposite procedure of abolishing all functional activity whatever. That some of the reported cures are premature, the case of Trenholme already cited abundantly proves. Even if it were proven that the Battey-Tait operation would produce the result claimed, we are met by the practical difficulty that in many cases which peculiarly appeal for help,—those with very large tumors weighing twenty, thirty, and even forty pounds,—it is impossible to remove the uterine appendages without doing hysterectomy.

The ovaries are often imbedded so firmly in the fibrous mass, that even the most careful dissection would not enable us to remove them with safety to the patient. The larger the tumor and the more need there is for relief, the less likely is the Battey-Tait operation to be either practical or successful. And even in the cases of smaller tumors, where their vaginal extraction is out of the question, we must conclude that the curative effect of the

Battey-Tait operation is "not proven." If it be asked how I account for the reported cases of such cures—or, rather, arrest of growth and subsequent shrinkage—I think many have to be thrown out like Trenholme's, because reported too soon. In others, the temporary modifying influence of such a radical invasion as this makes on the nutritive processes of the general system accounts for much. Until it is proved that fibrous and other tumors of the uterus arise from ovarian influences, I believe it is sound teaching to deny the justifiability of the operation for their arrest of growth. I think that this whole class of cases, those of uterine hemorrhage and uterine neoplasms, are not suitable for the Battey-Tait operation. The operation has been advanced on unsound physiological principles, and is, therefore, to be condemned as bad practice.

V. The last division of our classification embraces cases of diseases of the uterus and Fallopian tubes, for which it has been proposed to do the Battey-Tait operation.

As regards the chronic diseases of the uterus like endometritis of long standing for which this suggestion has been made, I do not know of any cases in which it has been carried into execution. The reasons already advanced for doubting the efficacy of the proceeding when the uterine symptoms are hemorrhagic or nervous, hold to an increased degree here. The effect that the subsidence of ovarian influence would have in modifying uterine malnutrition is so largely unknown, that no one would be justified in taking such a perilous leap in the dark. Until, therefore, much more definite information is gained, more especially by experimental pathology, on this point there is hardly any use in discussing this point. But a much more important set of cases are those of hydro-salpinx and pyo-salpinx, which Mr. Tait was the first to operate on. Here the conditions are quite different, for the diseased organs themselves are the seat of the proposed operations. They are inaccessible to direct local medication, and from their relation to the peritoneal cavity, are liable to be the foci from which much graver mischief may be set up. We owe very much to Mr. Tait for his great additions to our knowledge

on this subject, for before his writings appeared the whole region of the tubes in disease was practically an unknown country. It is remarkable that we have remained in ignorance on these points so long, for years ago Henle's description of the anatomy of the tubes might have suggested the extreme liability of the dilated part (the ampulla) becoming the seat of collections of fluid. And further, the occurrence of tubal pregnancy with its terrible mortality, should have drawn attention to the other possibilities of disease they possessed. Mr. Tait has done especially valuable work in calling the attention he has to the importance of gonorrhœa in producing salpingitis. His researches receive remarkable confirmation from the views expressed by Dr. Noeggerath on latent gonorrhœa in the female. The late Dr. Bumstead used to teach that gonorrhœa was more fatal to the human race than syphilis, and I certainly believe that is true in the female half of it. Gonorrhœa in the woman is apt to be a more serious disease than in the male, because of the different terminations of the Fallopian tube and the vas deferens. Once given an attack of gonorrhœa extending into the tubes on both sides, and the chances are overwhelming that the patient will be rendered sterile, and probably, if unrelieved, a confirmed invalid. Even though the inflammatory process stops in the tubes, and does not travel on, but gradually subsides, the products of inflammation are such as to render the the tubes useless as oviducts.

When the inflammation cannot be traced to gonorrheal infection, and even if that etiological factor can be excluded, if pyo-salpinx has once occurred, I think the above holds good. In opening up this field for the operation, Mr. Tait has done a great service, and his brilliant series of over sixty consecutive successful operations recently reported at the London Obstetrical Society meeting, shows how wide it is. When we come to his similar treatment for simple hydro-salpinx—dropsy of the Fallopian tubes—I am not inclined to agree with him. Tapping seems to me destined to play a very important part here, both in establishing the diagnosis, and also in curing the patient. The microscope may be made to subserve a most useful purpose in determining

the differential diagnosis between hydro- and pyo-salpinx, and consequently, I take it, in the justifiability of the operation. It seems possible to settle two things by tapping ; first, what is the nature of the fluid ? Are we dealing with a collection of pus, the consequence of an inflammation, specific or not, or with an effusion of serum ? Second, does the fluid come from the Fallopian tube ? In determining this second point my friend Dr. Frank Ferguson of New York has made a most admirable suggestion. As the result of a large number of examinations, he concludes that the presence of cells of columnar ciliated epithelium, is pathognomonic of salpinx. This test seems so simple and so rational that it should be resorted to in all suspected cases, and I hope its author will get the credit he deserves for advancing so beautiful and logical a diagnostic sign. In a given case, therefore, should we determine from the fluid withdrawn that it was from the Fallopian tubes, the further question of operation would depend on the presence or absence of pus in the fluid. If the case be one of hydro-salphinx the tapping is likely to prove curative and the operation is uncalled for. If it should prove to be pyo-salpinx aspiration is not likely to suffice, and after waiting for a short time to see, it would be right to resort to the Battey-Tait operation to effect a cure.

Conclusions.—Looking back over our analysis of the indications for which the Battey-Tait operation has been resorted to, I believe that the following deductions will hold good :

I. The operation is not justifiable for purely nervous symptoms independent of clearly marked evidences of organic ovarian disease.

II. It is not justifiable in cases of uterine disease or hydrosalpinx.

III. It is not justifiable—in the light of our present knowledge, —for the relief of uterine hemorrhage, or the arrest of the growth of uterine tumors.

IV. It is justifiable as a last resource—all other rational methods of treatment having failed : (*a*) In certain malformations and injuries of the utero-vaginal canal, rendering menstruation

impossible ; (*b*) In incurable organic disease of the ovaries ; (*c*) In pyo-salpinx.

The cure which the Battey-Tait operation offers to the large number of sufferers who constitute the fourth class, is one of the most glorious achievements of the surgery of our age.

WILLIAM M. THALLON, M.D.

NEW BOOKS AND INSTRUMENTS.

The International Encyclopædia of Surgery. A systematic treatise, etc. Edited by John Ashhurst, Jr., M.D., in six volumes. Vol. iv. New York : William Wood & Company, 1884.

The fourth volume of the work begins with a very meritorious article on *Injuries of Bones*, by Dr. John H. Packard, of Philadelphia. The pages bear throughout evidence of the author's thorough practical familiarity with the subject, and it is very agreeable to read, not only a compilation of what others have written, but also what Dr. Packard thinks on the several important details of his matter. As regards the theoretical part of some mooted questions, such as, for instance, Colles' fracture and fracture of the neck of the thigh, we find in Dr. Packard a commendable writer, inasmuch as he shuns all semblance of pedantry and learned buncomb. It would be difficult to name a modern author on fractures who, by attempts to be exhaustive and learned, does not occasionally become a bore. Long-winded disquisitions and quotations, instead of quickening the understanding, only muddle and exhaust the reader's intellect, and a subject like "fractures" must become "dry as dust," unless a clear-headed, practical writer knows how to infuse into it some of his own warmth and life.

There are, of course, a certain number of views on which issue might be joined with Dr. Packard; but who would quarrel about the "so-called antiseptic treatment" with an author who succeeds in both interesting and instructing his readers.

Dr. Packard justly condemns the "immediate" bandage, warmly recommends early passive motions, and in speaking of the treatment of union with deformity, strangely omits to mention McEwen's osteotomy with its brilliant results.

Among the improved means for treating fractures of the ribs, the application of the elastic bandage is not mentioned, It is far

superior to the irritating and unyielding adhesive strips, or even to a flannel bandage cut "bias."

The best parts of the article are those referring to the fractures of the clavicle, the radius, and the thigh.

Richard Barwell, of London, furnished the article on *Diseases of the Joints*. The difficult matter is treated with a master hand, and belongs to the best of the collection. Commendable is the systematic arrangement of the several parts under three headings : morbid anatomy, symptoms, and therapy. The attention paid to the pathology of the joints by the skilful writer, will captivate the reader's attention all the more, as the vivid, almost plastic description of trite, unattractive themes, shows the writer to be a man of varied knowledge and culture.

Mr. Barwell's nomenclature of synovitis seems to be unfortunate, because ambiguous. *Purulent* and *suppurative* synovitis is to most minds an identical thing ; yet Mr. Barwell names "*purulent*" a variety of simple synovitis, whereas the acute phlegmonous inflammation of the joints is called by him "*suppurative.*"

Evacuation of the joint in simple synovitis is justly recommended as a safe measure. In the preceding paper, Dr. Packard condemns it as dangerous and unjustifiable. But the *principle* of antiseptics is insisted on as a *conditio sine qua non.* Barwell has grave doubts as to Dr. Martin's assertion, that evacuation with subsequent elastic compression will always cure synovitis, even when the patient is directed to use his limb. The appropriate use of shampooing, massage, and gymnastics is recommended, not without a sarcastic side-glance at the pretensions and exaggerations of Mosengeil.

Suppurative and " urethral " synovitis form excellent chapters, full of valuable lessons based on personal experience.

In the treatment of synovitis by absorption, parenchymatous, or, as Barwell calls them, " interstitial " injections of different antiseptic agents with a tubular needle, are recommended. Hueter's similar, if not identical, efforts, made as early as 1873, are not mentioned.

The chapter on *Strumous Synovitis* shows evidence of excellent observation and careful study of phenomena, wide literary knowledge, and no mean skill in presenting this unattractive subject to the reader. Dr. Schüller's experimental studies regarding tuberculosis of joints provoke sharp criticism on the part of the author, who, however, is unable to furnish a better substitute for the microzymic theory.

Mr. Barwell gracefully acknowledges the priority of American surgeons in conceiving and successfully applying the principle of portable extension apparatus in joint affections. The objection to Sayre's adhesive-plaster contrivances seems to be well taken : skin irritation, loosening, and the necessity for frequent renewal being their undeniable accompaniments. The naïve Englishman does not see the mercantile value of just such defects.

Rheumatic, gouty, syphilitic, and neurotic joint affections all receive due attention, hip-disease having the distinction of a separate chapter. Among the many forms of hip-splint, permitting locomotion of the patient, Dumbrowsky's is declared to be the best.

The part on the treatment of anchylosed joints contains all that is worth knowing about the subject. Can it be accidental that McEwen's name does not occur at all among those recommending osteotomy ? What is known as McEwen's method, however, is minutely described by the author. A " misunderstanding " about priority seems to be lurking at the bottom of this matter.

A short but pregnant article by Dr. John Ashhurst, Jr., treats of *Excisions and Resections*. The value of the article is much impaired, however, by the author's anachronous attitude of negation respecting the value of the antiseptic wound-treatment. Dr. Ashhurst little thought, when quoting the words of the younger Moreau, how well they might be applied to himself. The elder Moreau having successfully practised excision of a diseased joint, "laid his views, based upon this experience, before the Academy of Surgery, definitely proposing the operation of complete excision of diseased articulations. In 1786 the same surgeon communicated to the Academy the history of a case, in which he had in that year successfully removed the whole shoulder-joint for disease, but neither of these communications seems to have excited interest. A third memoir, presented in 1789, met with almost universal disapproval and condemnation, the learned members of the Academy *finding it more convenient to deny, than to examine, the facts on which it was grounded, etc.*"

It is said on page 458 that chloroform was preferred by surgeons to ether in excisions of the upper jaw on account of its vapor being non-inflammable by contact with the hot iron. But finding that there is no risk of setting the patient on fire, Dr. Ashhurst has of late given the preference to ether.

It seems, accordingly, that the author does not appreciate the additional danger of pneumonia, caused by the use of ether in

operations done about the respiratory tract. Enormous masses of mucus are secreted, and becoming mixed with blood are aspirated into the bronchi to cause irritation.

Billroth's remarkable results in some fifty cases of exsection of the maxillæ treated with iodoform dressings, are not mentioned. König's celebrated case of total excision of the sternum for sarcoma is presented incorrectly ; there was no suppuration of the pericardium.

Langenbeck's method of resection of the wrist by one dorsal incision is omitted, although many surgeons regard it as far superior to Lister's procedure.

The author recommends in excisions of the hip-joint always to remove the trochanter. He is, on the whole, a warm advocate of excisions where properly indicated, and furnishes a number of histories of his own cases.

Dr. George E. Fenwick, of Montreal, winds up the subject with a short paper on *Excision of the Knee-Joint*, in which his peculiar manner of sawing the bones with a fine fretwork saw is described. Twenty-eight, mostly very successful, cases serve as a basis.

Tumors are disposed of by Mr. Henry Trentham Butlin, of London. The article is unpretentious, but that is its greatest merit.

Among the several views held about the etiology of tumors, the author inclines to the parasitic theory, because it "offers at the present moment greater *attraction* than any other" ; likewise does Mr. Butlin favor Gussenbauer's unconfirmed theory of the spread of tumor-infection to adjacent lymphatic glands by minute molecules, because the "statements"—contained in Gussenbauer's paper—" are made *in so confident a tone that his theory is very attractive.*" [1] And this by a countryman of Bacon !

Waldeyer's theory is accepted as the most convenient, and the classification based upon it. The different forms of tumor are then discussed *seriatim*.

As a compact elementary treatise of tumors, the article will undoubtedly fulfil very well the wants of the general practitioner.

A number of well-executed lithographic plates, some of them colored, accompany the article.

The late John A. Lidell's monograph on *Injuries of the Back*,

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[1] Reviewer's italics.

will not disappoint those in search of authentic information on this most difficult, and hence, it seems, most neglected, branch of surgery. The wealth of illustration, in the shape of a carefully selected casuistry of his own and of the experience of other surgeons, is knit together by the terse and clear text into a compact whole, fully up to the best work of the author.

Part I. treats of the injuries of the soft parts ; part II. of those of the vertebral column, the latter forming one of the best chapters of the essay. Dislocations, fractures, and gunshot injuries are considered in succession. As regards the last-named injuries, exception must be taken to the advice given by Lidell, that, "if the missile has lodged, it should be found and extracted, *if possible.*" This means that an attempt should be made any way,—a doctrine the soundness of which is gravely questioned by most surgeons of the present day.

The injuries and traumatic inflammation of the spinal cord and spinal nerves, together with the neuro-trophic changes arising therefrom—as, for instance, bed-sores, etc., are disposed of in part III. The article winds up with a consideration of the injuries of the sacrum and coccyx, and with a chapter on the remote effects of spinal injuries, as railway spine and others.

Malformations and Diseases of the Spine, by Mr. Frederick Treves, of London, form the subject of the last article of the volume.

Of the several modes of treating spina bifida, Morton's method of injecting the sac is recommended as the safest.

" *Cyphosis* " and " *lordosis* " are used to denote real *curvatures*, not angular deformities of the spine. *Spondylitis deformans* is the term used by Mr. Treves to denote chronic rheumatic arthritis of the vertebral column ; it is said that *its use is justified by custom.*

It is gratifying to read the author's contemptuous remarks about the absurd term " angular curvature," the blundering invention of an illiterate doctor.

The retention of the unmeaning term " Pott's disease " is earnestly urged, one of the most curious arguments for this being, that " the term commits the user to no particular pathological opinion."

The author shows good sense in disposing of the silly dispute about the constitutional or traumatic origin of Pott's disease with a very few words.

The pathology of the disorder receives very thorough attention, the symptomatology being mainly superimposed on the safe basis

of its morbid anatomy. Recumbency is declared to be the most important form of affording relief, and the host of mechanical appliances, including the different kinds of "jackets" and "plastrons," are subjected to a very wholesome, though not unfair, criticism. Indiscriminate suspension is justly declared to be dangerous; likewise is the wearing of the same plaster jacket for more than three months condemned.

On the whole, it can be said that Mr. Treves has produced a very creditable piece of work. [A. G. G.]

Practical Histology : A Manual for Students and Practitioners. By G. SIMS WOODHEAD, M.D., F.R.C.P.E., etc., with one hundred and thirty-six colored plates. American edition. Henry C. Lea's Son & Co., Philadelphia, 1884, 8vo, pp. 484.

The appearance of an American edition of this manual is to be hailed with pleasure, as giving in a clear, compact style the elements of pathological histology in such a manner as to be useful alike at the laboratory table and as a work of reference. But we believe that its chief excellence will prove to be as the constant companion of those working pathologists who look beyond mere naked-eye appearances in their study of morbid anatomy.

The first two chapters of the work give a brief but sufficient account of the instruments and reagents required for pathological examination, and explicit though incomplete directions are given for the opening and study of the cadaver. In the second chapter microscopical *technique* is detailed in a spirit of critical selection, and in view of immediate utility. The newer processes of staining, more especially as applied to bacteria, are given. We are pleased to see that Dr. Woodhead recognizes the merit of picro-carmine in these words: "By far the most useful reagent at present in the hands of the histologist is Ranvier's picro-carmine staining fluid "; and that he condemns the still too common practice of cutting sections by hand as a waste of time. The freezing microtome, with ether or salt, is now made so cheaply and yields such uniformly good results that its use should be taught all students. We have known students and even medical men greatly discouraged in their histological work, and almost tempted to give it up, because they could not acquire sufficient skill (and very few can) in cutting by hand. Even for hardened specimens, the microtome with an embedding mass is preferable. The time thus saved can be devoted to the much more important manipulations of staining and to *study* of the specimens.

The favorable mention of Valentin's knife (page 33) we can also endorse from personal experience, at least as regards the more consistent organs, such as the liver and kidney.

The normal and pathological histology of the various tissues, organs, and apparatuses is presented in nine chapters. In these chapters appear the almost inevitable (in view of the small size of the book) inequalities and omissions of the work. Still a better proportion, with an eye to practical requirements, might have been preserved between the various chapters. For instance, the pathology of the lung and of the kidney is quite fully presented in one hundred and thirty pages ; the spleen receives an undue share of attention in twenty-five pages. On the other hand, the pathology of the brain is disposed of in five (!) pages, and that of the entire nervous system in twenty-two. Nothing is said of such important lesions as encephalitis, myelitis, pachymeningitis, sclerosis, etc. Lesions of peripheral nerves, of muscles, of the skin, are not treated. That the pathology of the special nerves should be altogether omitted is more pardonable, considering the scope of the manual.

Tumors are well analyzed and illustrated in a chapter of sixty-four pages. The work closes with two chapters on parasites, animal and vegetable ; and in the last, bacteria receive suitable attention.

The illustrations, nearly all chromatic, are very good—many of them admirable. These, with the terse practical directions for the preparation of specimens, must constitute the strong feature of the manual. It is well printed, and, a strong point for a work-table book, it is so bound as to open and lie open easily and fully. [E. C. S.]

Transactions of the American Gynecological Society.
1883. Vol 8, pp. 276. D. Appleton & Co., New York.

In reviewing the Transactions of this Society for 1882, in the last number of the ARCHIVES, we drew attention to their extreme tardiness of apppearance, and it is therefore with the greater pleasure we note the improvement made in the prompt publication of the present volume. The book before us maintains the high excellence of the series in the matter of paper and type, although the publishers are changed. The proof-reading shows, perhaps, a little less care, notably in the report of the discussion on pp. 247 and 249, where several palpable errors have crept in. The excellent index of current Gynecological Literature, which has

been a feature in former volumes, is omitted this year, and the number of pages is considerably less than usual, owing to the average length of the papers read being less.

The Eighth Annual Meeting of the Society was held in Philadelphia on Sept. 18, 19, and 20, 1883, and fourteen scientific papers were presented :

I.　The Annual Address by the President, Dr. Gilman Kimball, of Lowell, Mass, was devoted to a " Biographical Sketch of Dr. Nathan Smith, Founder of the Dartmouth Medical College." The President traced in a most interesting way the main features in Dr. Smith's life, laying particular stress on the enormous amount of work accomplished by him. The author emphasized in particular how much the cause of medical education, more especially in the Eastern States, was indebted to Dr. Smith's heroic labors. He went on to portray his hero's eminent qualities as a surgeon, and wound up with an account of the famous case of ovariotomy, justly claiming that though subsequent to McDowell's, it was done without any knowledge of it. He therefore claimed for Dr. Nathan Smith a like degree of honor to that paid to the memory of the " Father of Ovariotomy."

II.　" Superinvolution of the Uterus," by Dr. Joseph Taber Johnson, is an interesting contribution to the study of this disease, first described by Sir James Simpson thirty years ago. He narrates four cases that have come under his own observation—in none of whom had treatment proved of any value. In the discussion, Dr. Fordyce Barker made an important contribution to the subject, by pointing out that cases of superinvolution of the uterus should be divided into two classes—one in which there was accompanying superinvolution of the ovaries, the other in which these glands retained their functional activity. In the first class his experience coincided with Dr. Johnson's, that all treatment was useless ; but in the second, treatment, especially galvanization of the uterus, had proved of great value in two cases he narrated, in both of whom pregnancy subsequently occurred.

III.　" Cleanliness in Surgery," by Dr. R. Stansbury Sutton, is a fair statement of the Doctor's observations in Europe, more especially with reference to abdominal surgery. The general consensus of opinion, both abroad and at home, to judge from the discussion, is in favor of the abandonment of the Listerian spray in this department.

IV.　" Hot Water in Secondary Hemorrhage after Pelvic Operations," by Dr. Albert H. Smith, draws attention to a hæmo-

static, the value of which he deems insufficiently appreciated. He narrates an interesting case in proof of his position, but neither in his paper nor in the discussion is an explanation of its mode of action offered. Dr. Barker raised the point that a certain time—sometimes even fifteen or twenty minutes—must elapse before you get the hæmostatic effect, and that therefore in some cases—he instanced one—it might not be of avail because of the delay. It seems to us before we can unreservedly accept Dr. Smith's conclusions, it is necessary to determine whether the hæmostatic effect is due to direct coagulation of the tissues—as in the use of the cautery—or to a reflex action producing contraction of the muscular elements of the vessels. We believe that the temperature of the water ($115°$ to $120°$ F.) that the Doctor gives, necessitates our adopting the second hypothesis, and if so, though often of great value, it will fail at times, like all other hæmostatics which act through the same mechanism, from defective innervation. We are therefore inclined to believe that this method is more applicable in secondary hemorrhage, than during an operation when the patient is profoundly anæsthetized, and all reflex action is wellnigh abolished.

V. "Some Points Connected with the Subject of Dysmenorrhœa," by Dr. C. D. Palmer, lays especial stress on the neurotic factor in the disease. He shows the insufficiency of the doctrine of its causation being due to mechanical obstruction, such as stenosis or flexion, as taught by Simpson and Marion Sims. While not denying the validity of this explanation in some cases, he instances others in which obstruction exists without dysmenorrhœa, and dysmenorrhœa without obstruction. He believes the vast majority of cases to be neurotic in their pathology, and he relies accordingly far more on the value of general than on local treatment. The gentlemen who participated in the discussion endorsed, in the main, the conclusions of the paper, and their general adoption would lead to a gratifying diminution in the number of incisions of the cervix.

VI. "A Rare Form of Abdominal Tumor: Three Cases, with One Specimen," by Dr. T. A. Reamy, describes three interesting cases of cystic tumors unconnected with the ovaries and uterus. Two were relieved by tapping; the third being shown by the post-mortem to take its origin from the omentum, numerous sarcomatous tumors having developed in the cyst wall. If the author's diagnosis is correct, that his two cases were not malignant, but would probably have become so if unrelieved—like the third,—

the question of resorting to the operation for their removal in the first place is of importance. But until more cases are put on record, his plan of tapping will probably be the safer practice.

VII. " On a New Mode of Operating for Fistula in Ano," by Dr. Edward W. Jenks, narrates two cases in which he incised the fistulous tracts in the usual manner, and after dissecting out the so-called pyogenic membrane or lardaceous and cartilaginous tissues along the fistula, he united the freshened surfaces. Externally the sutures were introduced as in operating for lacerated perineum extending through the sphincter ; while in the gut, where it was necessary to bring the divided surfaces in apposition, he used interrupted silk sutures. In this way union throughout was secured by first intention in ten days, instead of having to await the ordinary slow process of healing by granulation. In addition he claims that the risk of incontinence is greatly lessened. The operation is an eminently rational one, but in introducing it as something new the author is in error, for by referring to the *Chicago Medical Review*, some four or five years back, he will find this same plan described by Dr. E. P. Dudley ; in a subsequent article on the same subject, the suggestion is endorsed by Dr. Alex. J. C. Skene.

VIII. " Congenital Fissure of the Urethra with Exstrophy of the Bladder," by Dr. Henry F. Campbell, gives the history of a case of a little girl four years old, in whom this error of development is partially present ; the most interesting point in connection with this case being as to the proper time for operating to repair the deformity. The author intends to wait until after puberty before attempting to close the fissure, in order to gain the advantage of the patient's intelligent co-operation, and the increased room in the pelvis for his manipulations, and the more abundant tissues to bridge over the cleft.

Dr. Mann in discussing the paper urged (justly, we think) an early operation, on the ground that there was more space and tissue than is generally supposed, and because the parts would then develop more naturally.

IX. " The Management of Accidental Puncture and other Injuries of the Gravid Uterus as Complications of Laparotomy," by Dr. Charles Carroll Lee. The author has collected the literature of these rare accidents, comprising six cases, to which he adds one of his own. As a result of a careful study of the question, he comes to the conclusions that if the injury to the uterus also involves its contents, then Cæsarean section should be at once

performed ; if, however, the muscular wall simply be injured, as in his own case, it will suffice to carefully close the wound with carbolized sutures.

X. " Is the Extirpation of the Cancerous Uterus a Justifiable Operation ? " by Dr. A. Reeves Jackson. This paper contains an emphatic negative to this question, and the author bases his conclusion on a careful study of the recorded cases. He shows that more than seventy-two per cent. of the cases operated on, by the abdominal method, die at once ; while the remaining twenty-eight per cent. die inside of a year. The operation of complete removal by the vagina is not much better in its results. The author therefore justly concludes that the entire disease is never completely removed, and that the aggregate amount of suffering is not lessened by the operation, while the average length of life is curtailed. In the discussion the various participants narrated their personal methods of operating in these cases, but as they were methods of partial removal by the vagina, they did not conflict with the conclusions of the paper.

The only exception to this was Dr. Sutton, who stated he had done the complete operation five times, but he did not give his results.

XI. " A Study of the Etiology of Perineal Laceration, with a New Method for its Proper Repair," by Dr. Thomas Addis Emmet. The distinguished author begins by stating that all the ills usually supposed to follow a laceration of the perineum are really due to a transverse laceration, by which the parts lose the support afforded by the pelvic fascia. In order, therefore, to afford the patient true relief, some portion of the posterior wall of the vagina must be included in the line of union. It is not necessary to denude as much tissue anteriorly as is done in the ordinary operation of perineorrhaphy, as taught in Dr. Emmet's book. The author, in this new operation, denudes two crescentic lines of tissue : The upper one is given by three points,—the lower portion of the carunculæ on each side and a corresponding point on the posterior vaginal wall,—these three points being drawn together by tenacula first, and so determined ; the lower crescent is then formed in the ordinary way, but is less extensive than in the old operation. In the discussion Dr. Reamy wittily remarked that if this new operation ought to be substituted for the former one which Dr. Emmet has so admirably described in his work, it is fortunate that he has the opportunity to undo the great evil which he has done in teaching us a false or useless operation. It is dif-

ficult to criticise a plastic operation without seeing it done and examining the result, but it seems reasonable that Dr. Emmet has added an improvement by his new plan in cases of extreme rectocele. At the same time we cannot see why in the ordinary operation his object is not attained far more simply by a single crown suture, which is passed through the upper angles of the denuded tissues and includes the lowest point in the posterior wall in its grasp. The same fault which characterized Dr. Emmet's contribution last year " On a New Method of Treatment for Diseases of the Urethra," is evident in this paper—the sweeping condemnation of all other methods, except the new one about to be brought forward. It is, however, consoling to reflect that in this instance the teacher whom the Dr. Emmet of to-day will first have to refute before his new dispensation can be accepted, is the Dr. Emmet of twenty years ago.

XII. "Remarks on Chronic Abscess of the Pelvis," by Dr. William H. Byford. This is one of the most interesting and original papers in the present volume. The author traces the changes that take place, more especially in cellulitis and hæmatocele after resolution has occurred. He traces the natural course of the abscess if unrelieved—the routes it will take to discharge, and the chances of subsequent cicatrization occurring. He finds that this depends mainly on the conditions as to compression of the surrounding tissues and the state of the abscess wall. He shows that suppuration is kept up by the granulations lining the wall of the abscess, and if these are thoroughly removed and the walls kept in proper opposition, union by cicatrization occurs. He therefore advocates the practice of free drainage per vaginam in most of these cases, together with the removal of the tags of granulation tissue projecting into the abscess cavity by means of a curette.

The paper is a model of accurate and minute observations and logical thinking. In the interesting discussion which followed, Dr. Byford's views were strongly endorsed, the additional point being made by Dr. Thomas : he believed in following Mr. Tait's plan of abdominal drainage in those cases where the safer method of Dr. Byford was inapplicable.

XIII. " Ergot : the Use and Abuse of the Dangerous Drug," by Dr. George J. Engelmann. The author preaches a sermon from the text that " ergot should only be given in the non-gravid uterus." This so exactly corresponds with the views expressed by another member of the Society (Dr. Johnson), in a paper read

at the previous meeting, that it would have been better had the author at least alluded to the coincidence. The style of this paper cannot be commended, for it is quite unnecessary, in protesting against the evils resulting from prolonged tonic spasm, to do so in a manner strongly suggestive of the clonic variety.

XIV. " Rectal Disease Considered in its Relations to Disorders of the Uterus," by Dr. R. B. Maury, is the paper presented by the new Fellow of the Society. He shows first the essential unity of the vascular supply of the bladder, uterus, and rectum; how they are all supplied, almost exclusively, from the same arterial source; how the venus plexuses all terminate in one channel, the internal iliac vein; and how closely connected they are by this nervous supply. From these facts he deduces the close relations of the parts in disease, in hyperæmia, in chronic inflammation, and chronic ulcer of the rectum. Perhaps the most interesting point in his excellent paper is the explanation he offers for the greater frequency of occurrence of rectal ulcer in women than men.

A memoir of the late Dr. James Dowling Trask, of Astoria,—a beautiful tribute to the memory of a good man,—by Dr. Fordyce Barker, concludes the volume.

This volume as a whole is worthy of the Society from which it emanates. The papers are mainly clinical, and consequently of great practical value ; the discussions which they provoked are generally to the point, and give the ripe experience of men—the best qualified to teach us on these subjects. [W. M. T.]

ARCHIVES OF MEDICINE.

Original Articles.

TRACHOMA AND THE ETIOLOGY OF JEQUIRITY OPHTHALMIA.*

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THE motive for presenting to this society a paper on a disease so common as trachoma, is furnished by two circumstances, *i. e.*, the recent introduction by Dr. de Wecker of Paris of a new therapeutic agent for the treatment of this rebellious affection, and a new view of its etiology advanced by Prof. Sattler of Erlangen.

If we may measure the importance of a disease by the frequency with which it is met in practice and by the amount of controversy to which it has given rise, then *trachoma* or *granular conjunctivitis* deserves a conspicuous place in medical literature.

Permit me to say, at the outset, that whatever criticisms I may make regarding the histology of the conjunctiva and the tissue changes in granular conjunctivitis, are based upon examinations of eighteen healthy human conjunctivas at ages varying from the time of birth to the thirty-first year, and forty-seven cases of trachoma examined at different periods of the disease during the past six years. The chapter on the *etiology of jequirity* is the out-

* Paper read before the New York County Medical Society, April 28, 1884.

come of experiments made on a large number of rabbits
and dogs ; and that part of the paper which deals with the
bacterial view of trachoma, is based upon examination of
thirty-seven cases of trachoma with this end in view.

ETIOLOGY.

Granular conjunctivitis is developed most frequently by
contagion,—by the transportation of secretion from the eye
of a patient with the disease to a healthy eye. Yet, we
know that blennorrhoic pus may give rise to granular con-
junctivitis in another, although it may never develop in the
original eye ; and cases of this disease certainly do occur in
persons who have never been brought in contact with a patient
who is the subject of granular conjunctivitis. Von Græfe
maintained that the contagion was transmitted through
the atmosphere. This much we do know, that the disease
in question does sometimes occur epidemically among
individuals living under the same bad hygienic conditions.
But just why blennorrhœa will develop in one case into
trachoma and not in others is not explained, in fact, by the
supposition that the one disease is *bacterial* in its origin and
the other not. *The numerous examinations and cultures of
blennorrhoic pus have convinced me of the pretty constant
presence of a micrococcus, but that there is no morphological or
physiological difference between it and the micrococcus found in
trachoma pus ;* and we are forced to speculate that trachoma
is a simple blennorrhœa produced by bad hygiene, which
so modifies the organism that the disease assumes a par-
ticular condition. To what degree we should regard
diathesis or congenital predisposition responsible for tra-
choma is a question of great interest and importance. If a
particular bacterium were responsible for the disease,—this
bacterium, like that of tuberculosis and typhoid fever, etc.,
inhabiting the air,—we could still reconcile ourselves to the
immunity or rather escape of many individuals from tra-

choma. However, we have, in the idea of the bacterial origin of trachoma, a hypothesis which experimentation alone must prove or disprove.[1] Prof. Sattler, who first announced the discovery of a micrococcus in the trachoma tissue, claims to have succeeded in reproducing the disease with the elements derived from the culture of the parasite, which he considers to be the generator and vehicle of the contagion.

My own investigations are diametrically opposed to Sattler's view. I have succeeded in cultivating in sterilized gelatine bouillon, and blood-serum culture media, a micrococcus variety derived from the trachoma secretion or pus, but I have never been able to reproduce with pure cultures (for we often find many different forms of bacteria in these secretions) a disease resembling trachoma. I have made diligent examinations of over 1,500 sections derived from thirty-seven cases of trachoma—the piece of tissue for examination having been excised during life, and placed at once in strong alcohol, and subsequently embedded in celloidin, and the thinnest sections made with a microtome, and subjected in many cases for a longer time than usual to the action of one of the aniline coloring agents, methyl blue, methyl violet, fuchsine, Vandyke brown, Hoffman's purple, and decolorized with alcohol, or acetic acid, and mounted in glycerine or balsam without chloroform, an Abbe's condenser being used in every instance.

Sattler[2] acknowledges that micrococci are absent in some cases from the secretion in trachoma at certain periods of its course, when, he says, the secretion is not contagious. He states that from seven to eight days after inoculation of the selected culture medium (blood serum) with secretion from trachoma, there spring up at the site of inoculation a

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[1] Bericht über die Versammlung der Ophthal. Gesellschaft, Heidelberg, 1882.

[2] *L. c.*, p. 173.

few pale, yellowish points ; and that he found exactly the same micrococcus in the trachoma tissue as in the secretion. He speaks of the micrococci clinging to the exterior of the nuclei.[1] If these micrococci actually do exist in the trachoma tissue I have never been able to find them. It is so very easy to be mistaken as to the identity of certain bacteria, especially those of the micrococcus tribe. We know how closely micrococci are simulated by nuclear detritus in the blood, by the " Mastzellen " of Ehrlich—a large granular cell found in inflamed tissues—and although these objects may usually be distinguished from micrococci by their rarely being of uniform size as micrococci are, and by their reaction to certain staining agents, yet the positive proof, after all, is usually only furnished by cultivation outside of the body. In examining this question for myself, I proceeded as follows :

After thoroughly washing the conjunctiva of a given case of trachoma with a solution of bichloride of mercury, too weak to destroy micro-organisms after so short an application, this being used only for its mechanical effect to remove all secretion, and because it would not be likely to be itself contaminated with micro-organisms as in case plain water were used. The scissors and forceps were sterilized by heat, and one or more trachoma granules were excised and placed in the sterilized gelatine bouillon, and transferred to the incubator at a temperature of 35° to 36° C. In some instances, despite my precautions, the culture medium was contaminated by a variety of bacteria, but inoculation of rabbits' and dogs' eyes with these failed to produce trachoma. But in a large number of instances the culture medium remained free from bacteria for several weeks, until they were thrown away. However, I am still engaged upon this subject, because I believe it requires further investigation.

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[1] " Bericht der Ophthal. Gesellschaft," Heidelberg, 1881, p. 24.

Experiences must be multiplied, and if they conform to one another, then only can we have a true conviction. This fact remains: inoculation with trachomatous pus produces in some cases an acute inflammation, which may disappear without leaving a trace of mischief, or it may give rise to granular conjunctivitis. If there were a specific element you might say it should always reproduce the same disease and not another. But even such an argument would not dispose of the idea of the specific character of the disease in question. As already stated, I have found micrococci in blennorrhoic and trachomatous pus (if I may take the liberty with the term), and there is nothing unusual in this circumstance, but although apparently identical morphologically, I have not succeeded in establishing by cultivation a physiological difference—*i. e.*, in isolating a micrococcus or any other variety of micro-organisms which is capable of producing trachoma; nevertheless, it may exist. And even should Sattler's views be disproved by others, they do not deserve to be ridiculed with criticism based upon hypothetical reasoning. His analyses are evidently honestly meant, and have been fairly conducted, and should be treated in a more liberal spirit than has been accorded to similar views of other writers in connection with other diseases.

It is a curious circumstance that purulent conjunctivitis, and especially blennorrhœa, may give rise to trachoma; and in view of recent developments, or rather contradictions, it would be less hypothetical to maintain the existence of a close relationship between granular conjunctivitis and other forms of conjunctival inflammations; and when we find something special, as in trachoma, to refer this to the individual and not to the disease itself. But in what degree a certain diathesis is responsible for this disease, it is difficult, even in the presence of statistics, to speak with posi-

tiveness. At any rate it is a significant circumstance that the granulations attack by preference individuals the state of whose lymphatic system predisposes them to the production of certain diseased conditions. Trachoma is comparatively rare among those whose means permit them to live under good hygienic conditions, and in healthful climates; and these conditions generally influence trachoma favorably. This, of course, is characteristic of every disease which is due to a mischievous diathesis or deterioration of the organism from whatever cause. The same may perhaps be said respecting the *influence of race*.

Dr. Swan M. Burnett,[1] of Washington, D. C., has directed attention to the immunity of the negro from trachoma. In a letter which I have recently received from this gentleman, he stated that he intended to publish shortly the results of his investigation among negroes in connection with the disease in question. Dr. B. informed me that he had examined one thousand negroes without finding a single example of trachoma among them. But it would be highly important and interesting to investigate the conditions under which these people live. Whatever influence race may exercise there can be little doubt that geographical, or, better perhaps, climatic causes and unfavorable hygienic surroundings play a still greater part in the etiology of this disease. Trachoma has been styled "*the Celtic disease*"; but here, too, we should consider the conditions just alluded to.

Sæmisch[2] directs attention to the increase in the extent of trachoma along the Rhine, from its source to its mouth. Although this disease is said to almost never originates in Switzerland, we should refer this immunity to the climatic rather than the mountainous condition of the country, because mountainous regions do not furnish any protection against the spread of the affection.

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[1] Report of the Fifth International Ophthalmological Congress, 1876.
[2] Sæmisch und Græfe: Handbuch, Bd. iv, p. 66.

In Southern Russia, Mandelstamm [1] states that trachoma constitutes 35 per cent. of eye affections. And this large percentage he traces to the small and filthy homes, especially among the Israelites. There, too, *struma* and *trachoma* go hand in hand. In New York City and Brooklyn, estimated from the reports of the eye and ear hospitals of these cities during the past ten years, trachoma constitutes five per cent. of the cases of eye disease.

If I deny the existence of a bacterium in the trachoma tissue, it is still very probable that *micrococci* are the real *provokers* of the contagion, and that these should be sought in the secretion, and not in the conjunctival tissues; and, further, that a particular diathesis, and climatic and hygienic conditions, also play a very significant part in the production of this serious disease.

During the past few years the question of trachoma has been very freely ventilated, and yet, despite the prodigious amount of scientific investigation devoted to the subject, opinions are still far from being unanimous in regard to the nature of the process. By some it is maintained that in trachoma we have to do with a simple inflammatory process; while others believe that it is an inflammation with new formations, to be considered as a disease *sui generis*, with a specific course.

In order to understand the development of the structures which are considered characteristic of trachoma, it will be desirable to recall the normal condition of the human conjunctiva, and I may be excused for briefly referring to it. In the fresh state, the conjunctiva is of a delicate pink color, varying in thickness according to the part which it covers—*i. e.*, from the margin of the lid, backward, it increases in thickness, again becoming attenuated where it is reflected upon the globe; the increase in thickness being

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[1] *Græfe's Arch. f. Ophthal.*, Bd. xxix, 1883, p. 56.

referable to the development of adenoid tissue. The conjunctival surface is not smooth; it is traversed everywhere by furrows which vary in depth, and cross each other in such a manner as to present the picture of a continuous network when viewed from the surface. These are the so-called papillæ or papilliform elevations. But this papillary condition of the surface only manifests itself to the unaided eye at that part of the conjunctiva which extends from the point of transition from conjunctiva of the lid to its point of reflection on the eyeball, known as the cul-de-sac.

It is easy to convince one's self of the unevenness of the healthy conjunctival surface, and that this is due to invaginations of its epithelium, and that this is most pronounced in the cul-de-sac. Reich, Sattler, Stieda, Baumgarten, Jacobson, Jun., Mandelstamm, Ræhlmann, and others agree that this condition is present in the normal conjunctiva; the point of dispute being whether these invaginations are glands or not. Stieda, Sattler, and, quite recently, Ræhlmann have shown that in the new-born there is not a trace of adenoid tissue in the conjunctiva; that beneath the epithelium there is a thin layer of fibrillar connective tissue devoid of cellular deposit; so that in a child of six months, the thickness of the epithelial and fibre-layer together should not exceed 0.05 mm.—0.1 mm.[1] In the adult the conjunctiva is 0.2 mm.—0.3 mm. thick, and in the upper third of the tarsal portion and in the cul-de-sac it may attain to a thickness of 0.4 mm.—0.5 mm.[2]; this increase in thickness being due to the development of adenoid tissue. The most recent writer on trachoma, Ræhlmann, maintains that the possibility of the development of this disease depends upon the existence of the adenoid layer, the first indication of which he found in the conjunctiva of a child of two months.

Bruch was the first writer to describe lymph follicles in

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[1] Ræhlmann: *Græfe's Archiv für Ophthal.*, Bd. xxix., Abth. ii., 1883, p. 74.
[2] *Ibid.*

the conjunctiva of the inferior eyelid of the ox.[1] Stromeyer[2] described follicles in the conjunctiva of domestic and wild animals, and he spoke of them as pathological products. Henle[3] calls them trachoma glands. Krause[4] held the lymph follicle which he found in the conjunctiva of the rabbit, fox, and birds, to be physiological formations, and considered them identical with the solitary lymph follicle in Peyer's patches in the intestinal canal. Kleinschmidt[5] and Huguenin[6] found such follicles in the conjunctiva of man. According to the observations of Waldeyer, Sattler, and Ræhlmann, lymph follicles are not present in the normal conjunctiva. Baumgarten, on the other hand, maintains that lymph follicles are also met with in the normal conjunctiva, but his evidence is not convincing. Goldzieher[7] has recently published some observations on the occurrence of lymphoid growths in the conjunctiva, from which he concludes that lymph follicles are normal constituents of the conjunctiva ; but Goldzieher, it seems to us, fails to sustain his argument against Waldeyer and Sattler. Now, if the follicle be a pathological product in the conjunctiva of man, what proof have we of its presence as a physiological formation in animals. A few years ago when I occupied myself with the study of the conjunctiva of pigs, birds, and rabbits, I was not successful in demonstrating the presence of follicles in every instance, even after a very careful search and with the assistance of others more experienced than myself, and I have since come to believe that the presence of this formation in the conjunctiva is directly traceable to the

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[1] Bruch : *Zeitschrift für Wissenschaftliche Zoölogie*, Bd. iv., p. 297.

[2] Stromeyer : '' Beitrage zur Lehre von der granulösen Augenkrankheit.''— *Deutsche Klinik*, Heft 25, 1859.

[3] Henle : '' Eingeweidelehre,'' Bd. ii., p. 732.

[4] Krause : '' Anatomische Untersuchungen,'' Hannover, 1861, p. 154.

[5] Kleinschmidt : '' Ueber die Drüsen der Conjunctiva.''—*Græfe's Archiv f. Ophthal.*, Bd. ix., Ab. 3, p. 157.

[6] Huguenin : '' Ueber die Trachomdrüsen oder Lymphfollikel der Conjunctiva,'' Zürich, 1865.

[7] Goldzieher : *Hirschberg's Centralblatt f. Augenheilk.*, Sep., 1882.

manner in which these animals are cared for, and that in
them as well as in man it is a pathological product. It is at
least found chiefly among such animals as rabbits and pigs,
which are herded and come in bodily contact, whereby oppor-
tunity is offered for the spreading of the disease through in-
fection and direct transfer, while in birds and cats it is
seldom met with.

Synonyms : Granular conjunctivitis, which seems to be the
most appropriate term to apply to this condition is. also
known as *trachoma, ophthalmia trachomatosa, granulations* of
the *eye-lids, ophthalmia Ægyptica, O. militaris, O. bellica, O.
des armées, O. contagiosa*, and the modern addition, *conjunc-
tivitis follicularis*. In its etymological sense, *trachoma* (from
τραχωμα, a roughness) can be applied to any of the diseases
of the conjunctiva which render the surface of the latter
uneven. In view, therefore, of the follicular and granular
processes being classed together, and as only a certain per-
centage of these cases result in retractions of the palpebral
conjunctiva, the term trachoma would apply more appropri-
ately to them, but this would not be so readily understood
as the designation, conjunctivitis granulosa in stadio cica-
triceo.

When we examine the text-books on ophthalmology and
ophthalmological journal literature, we are perplexed by the
laxity in regard to the definition of trachoma from a
clinical standpoint. One author designates as " *trachoma* "
what another describes as " *follicular catarrh*." Schweig-
ger [1] describes as "trachoma granule " an accumulation,
immediately under the epithelium, of lymph-cells without,
as a rule, any enveloping capsule of connective tissue.
Arlt [2] considers trachoma identical with the chronic blen-
norrhœa of the conjunctiva in ophthalmia Ægyptica, mili-
taris, contagiosa, and " granulations." Others speak of

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[1] Schweigger: Text-book. Translated by Porter Farley, M.D., p. 276.
[2] Arlt: " Klin. Darstellung der Krankheit des Auges," 1881.

granulations when they have to do with so-called "follicular formations," which they designate as "follicles" or "granulations." Others, again, regard granulations as papillary proliferations. Sæmisch[1] considers the so-called trachoma granule as distinct from follicles as well as proliferated papillæ.

A large number of the older writers regarded the trachoma follicle as a normal, a physiological formation: Krause,[2] Huguenin,[3] Frey,[4] Hessling,[5] and Kleinschmidt.[6] More recent writers, among whom may be mentioned Sæmisch and De Wecker, deny that follicles occur in true trachoma. On the other hand, a great number of most competent observers regard the follicle as the basis of trachoma: Decondé,[7] Bendz,[8] Stromeyer,[9] Blumberg,[10] Horner,[11] and others. Again, regarding the nature and structure of the trachoma follicle, opinions are very divergent; some writers holding that the trachoma follicle is identical with the closed follicle of the intestine, and consists of a glandular formation with a closed enveloping membrane: Henle,[12] Stromeyer.[13] On the other hand, Mandelstamm,[14] Remy,[15] Schweigger,[16] De Wecker,[17]

[1] Græfe und Sæmisch : " Handbuch der gesam. Augenheilkunde," Chap. iii.

[2] Krause : " Anat. Untersuchungen," Hannover, 1861, p. 154.

[3] Huguenin : '' Ueber die Trachomdrüsen oder Lymphfollicle der Conjunctiva," Zürich, 1865.

[4] Frey : Handbuch, p. 695.

[5] Hessling : "Grundzüge der Gewebslehre," Leipzig, 1866, p. 220.

[6] Kleinschmidt : *Græfe's Archiv f. Ophthal.*, Bd. ix., 3, p. 157.

[7] Decondé : Annales de la Société de Médecine de Gand, Juin, 1840, p. 122.

[8] Bendz : Compte-rendu Congrés d' Ophthalmologie, Paris, 1858, p. 236.

[9] Stromeyer : *Deutsche Klinik*, 1859, Bd. xi., p. 246.

[10] Blumberg : *Græfe's Archiv f. Ophth.*, Bd. xv., 1, p. 142.

[11] F. Horner : Die Krankheiten des Auges in Kindisalter in Gerhardt's Handbuch der Kinderkrankheiten. Bd. v. Abth. 2. p. 305.

[12] Henle : *Zeitschrift f. ration. Med.*, Bd. viii., p. 227.

[13] Stromeyer : *Deutsche Klinik*, 1859, Bd. xi., p. 246.

[14] Mandelstamm : *Graefe's Archiv f. Ophth.*, Bd. xxvii., 3, p. 106.

[15] Remy :

[16] Schweigger : *Loc. cit.*

[17] De Wecker : Text-book. Translated by L. Forbes, M.D., p. 82.

and others declare that the trachoma follicle has no enveloping membrane. Sattler[1] objects to the view held by Sæmisch, regarding the new-formation of lymph follicles in the conjunctiva, in his so-called follicular conjunctivitis, on the ground that it is without an analogue in pathological anatomy. Nevertheless, we do find examples of the pathological appearance of follicles in places where they never occur in a normal condition (Virchow and Bœtcher[2]), and analogous in structure to the follicles of the intestine—*i. e.*, in the liver in leuchæmia; and Birch–Hirschfeld[3] alludes to the circumstance that the hyperplastic lymphomata are not infrequently complicated with the development, in the connective tissue of different organs, as the liver, kidney, etc., of small nodes, corresponding in structure to lymph-gland tissue, and that these secondary lymphomata are seen in variola, scarlatina, leuchæmia, and pseudo-leuchæmia.

We see, therefore, a large number of distinguished observers unwilling to agree with one another as to the actual nature of trachoma, and in consequence of this disagreement there can be no certainty in regard to prognosis, and the treatment must be empirical.

THE NATURE OF TRACHOMA.

The writer's observations are based on the examination of forty-seven cases of trachoma. In every case several of the prominences of the conjunctiva were excised, and in several instances the entire conjunctiva was examined after death. In all cases, at first, the epithelial surface was found smooth, not thickened. The adenoid tissue was infiltrated with lymphoid cells, but the latter were not uniformly distributed, but in places it extended deeper into the tissue and was more closely arranged. At first there is a circum-

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[1] Sattler : Bericht über die Heidelberger Ophthal. Gesellschaft, 1881, p. 19.

[2] Virchow and Bœtcher: *Virchow's Archiv*, Bd. xviii., p. 22.

[3] F. v. Birch–Hirschfeld : " Lehrbuch d. path. Anatomie," 1877, p. 131.

scribed arrangement of lymphoid cells, as represented in fig. 1,[1] and is then without a limiting membrane. The tissue between the foci of infiltration may be quite normal, or, at most, slightly hyperæmic. As a rule, at an early period of their existence the foci of infiltration have a round contour, and the cells at the periphery are smaller and more closely packed together than in the interior, but there is as yet no true anatomical boundary. Under sufficient power, the interior of the young follicle has a distinct reticulum. It is a framework consisting of abundantly branching pale fibres with gangliform swellings, and in places stellate,[2] branching points with pale, oval nuclei. At this early stage, therefore, the follicle has an envelope only in the sense that the cells are most closely aggregated at the periphery. At a later period the small cells at the boundary assume an oblong shape, and look like a string of beads; and finally the fibrous boundary of the follicle is formed by the cells at the periphery being drawn out into fibres; however, this fibrous boundary is not always found symmetrically developed, but some part of the circumference is bridged over with small round cells, and frequently the follicle is found to communicate with the surface of the conjunctiva through a breach in the enveloping capsule. The young follicle represents a pretty solid nodule, and cannot be easily emptied of its contents by puncture from the conjunctival surface. Leber,[3] on the contrary, maintains the opposite view. As the disease advances, the fate of the follicle will be varied according to the intensity of the inflammation, and its contents may become obliterated or undergo softening.

Frequently the follicles are confluent, and when this con-

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[1] I am unable to furnish the wood-cut in time for the printers, but on page 31 of Græfe and Sæmisch's " Handbuch," vol. iv., will be found a similar picture.

[2] Ræhlmann: *Græfe's Archiv*, Bd. xxix., 2, plate v., fig. 13; also Jacobson, Jr.: *Ibid.*, Bd. xxv., 2, p. 169, plate v., fig. 7.

[3] Leber: Bericht der Ophthal. Gesellschaft, Heidelberg, 1878, d. 12.

dition extends over a considerable space, the typical boundary described above will not be found. Sclerosed, thickened vessels, as found by Sattler, in the interior of the follicle are developed into fibrous connective tissue ; and he alluded to this as the most important circumstance in the connective-tissue transformation. According to Ræhlmann this transformation into connective tissue as issue of the follicle formation in trachoma is not rare, but he states that another fate awaits the follicle which is the more frequent —*i. e.,* the formation of *follicular ulcers.* When the conjunctival cells undergo mucous degeneration, the conjunctiva assumes a soft, gelatinous condition—the gelatinous (*sulziges*) trachoma of Stellwag. In recent trachoma there is often a vascular network about the follicle. Iwanow[1] and Mandelstamm[2] deny the existence of blood-vessels in the interior of the trachoma follicle. According to others it has a few blood-vessels. Blumberg,[3] Sattler, Horner, Stellwag, Leber, Jacobson, Jr., and Ræhlmann[4] have shown that the follicle does contain blood-vessels ; and injected eyes of animals prove the existence of blood-vessels. I have repeatedly seen vessels, and quite large ones, in the interior of trachoma follicles—fig. 1. The vessels are generally found just inside the wall of the follicle, but only a few firm capillary vessels project into its interior, and these branches are so small that they are only visible by high power. *In the neighborhood of the follicle blood-vessels are often numerous, but their presence and number will depend upon the stage and duration of the disease.*

Some observers have found the follicle of the conjunctiva surrounded by a lymph-sinus. Frey[5] and Wolfring[6] have

[1] Iwanoff : Bericht der Ophthal. Gesellschaft, Heidelberg,1878, pp. 19 and 21.

[2] Mandelstamm : *Græfe's Archiv*, Bd. xxvii., p. 106.

[3] Blumberg : *Græfe's Archiv*, Bd. xv., p. 143.

[4] Ræhlmann : *Græfe's Archiv*, Bd. xxix., plate iv., fig. 7.

[5] Frey : *Virchow's Archiv*, Bd. xxvi.

[6] Wolfring : *Græfe's Archiv*, xiv., 3, p. 181.

described and pictured lymph-vessels in the periphery of the follicle of dogs; and C. Langer [1] has shown in injected preparations, that the follicle is in intimate relationship to the roots of the veins, in that the follicular substance is disposed about these roots.

When the follicles are isolated the tissue-changes are generally limited to the site of the follicle and its immediate vicinity, and the interlying tissue may be free from changes. But generally we have to do with various conditions of catarrh, and these inflammatory changes take place in the surrounding tissue and complicate the process.

Ulceration and cicatricial formations are a more frequent issue of the follicular conjunctival inflammation than induration through new connective-tissue formation, and the two processes generally occur together—ulceration and cicatricial formation on the surface, and connective-tissue sclerosis in the deeper parts. We have also analogous results in inflammation of other mucous tissues, *i. e.*, throat, mouth, intestines, and vagina. The scars which develop on the surface of the conjunctiva are frequently radiate, and sometimes pigmented. As a consequence of the connective-tissue formation in the deeper parts of the conjunctival membrane, the surface of the latter assumes a characteristic appearance over all that portion previously occupied by the follicles; then we see a cicatricial surface with thin epithelial covering, and the gland and epithelium are so far affected as to surrender their power to act as physiological organs. And thus by ulceration of the follicles and the formation of connective tissue in and about the follicles the entire adenoid tissue invaded by the follicular formations is destroyed. So long as the adenoid tissue remains uninvaded, it serves a useful purpose in preserving a certain degree of softness to the conjunctiva; but when its tissue is destroyed in the

[1] C. Langer: *Wiener med. Jahrbücher,* 1878, p. 336.

manner just referred to, we have a hard, irregular, unyield-
ing, dry scar surface. This is of course the extreme result
of the trachoma process, and while these changes may be
brought about in a few months in acute cases of the disease,
in the majority of instances years are required for the chronic
form to reach the same. In chronic trachoma, the follicles
are at first less dense, but aggregated in isolated heaps, and
as some of these follicles are obliterated, others take their
places ; and so the creation of new follicles and connective-
tissue formations and sclerosis goes on, so that in the chronic
cases the latter changes attain to an unusual degree. The
action of jequirity finds a partial explanation in the assump-
tion that the existing follicles are destroyed, and the
further development of others prevented, by the serous in-
filtration of the conjunctival tissue which it occasions. At
least we can explain the occasional enormous thickening of
the conjunctiva by the continuance of the connective-tissue
formation about the new and old follicles, the latter being
crowded into the tissue by the new growths, the connective-
tissue formation going on, if not arrested by natural or arti-
ficial agencies, so long as adenoid tissue remains. I do not
mean to maintain that trachoma produces these extreme
changes in the whole conjunctiva in every instance, even
when the disease is left to itself. The most common
issue, when the disease is appropriately treated, is that
the trachoma process is confined to a few places in the con-
junctiva, and when the stage of scar formations is reached,
the disease ends. In the great majority of cases, however,
the process goes on.

The hypertrophic cock's-comb-like fold of the cul-de-sac
has recently been classed by Stellwag[1] among the amyloid
tumors. He maintains that his gelatinous trachoma (*sul-
ziges Trachom*) is identical with amyloid tumors,—*i. e.,* that

[1] Stellwag von Carion : '' Abhandlungen aus dem Gebiete der prak. Augen-
heilkunde,'' 1882. Published by Braumüller, in Vienna.

the latter proceed from the former. He defines amyloid tumors as polypoid swellings; that they are partly clodded masses which, in later stages of the process, always rest upon a cicatricial retracted base or upon an actual pedicle, and, as a rule, spring from the semilunar fold or caruncle. In support of his assumption that these amyloid tumors proceed from trachoma, Stellwag adduces that he saw them "appear and grow on conjunctivas which were unquestionably the seat of trachoma." But this is certainly no proof that they owe their existence to trachoma, and more substantial evidence is necessary to establish the etiological connection. Kubli [1] observed in a series of thirty cases of these tumors in the conjunctiva, fourteen in which antecedent or accompanying trachoma could be excluded; so we see these amyloid tumors do occur independently of trachoma.

The adenoid, epithelial invaginations of the conjunctiva in trachoma were first described by Berlin and Iwanow, and declared by them to be new growths and pathognomonic of trachoma. It is generally conceded that these structures exist, but respecting their significance opinions diverge. Reich[2], de Vincentis[3], Ciaccio[4], and recently Baumgarten, have shown that these glands of Berlin and Iwanow are physiological structures.

At birth no adenoid tissue exists in the human conjunctiva; it develops in old age. Stieda[5], Sattler[6], Waldeyer[7], deny that glands exist in the human conjunctiva, and refer these formations to invaginations of the epithelial surface at the base of the furrows on the surface of

[1] Kubli: *Archives of Ophthal.*, 1882, vol. xi., No. 2, p. 167.

[2] Reich: *Græfe's Archiv f. Oph.*, Bd. xxi., p. 1.

[3] De Vincentis: *Nagel's Jahresbericht*, Bd. iv., p, 247.

[4] Ciaccio: Quoted by Henle in his "Handbuch der Anatomie des Menschen," Bd. ii, p. 731.

[5] Stieda: *Archiv f. mikr. Anatomie*, Bd. iii., p. 357.

[6] Sattler: *Græfe's Arch. f. Oph.*, Bd. xxiii., 4, p. 1.

[7] Waldeyer: Græfe u. Sæmisch, "Handbuch," Bd. i., p. 240.

the conjunctiva. I have never found actual tubular glands in the human conjunctiva; but if we regard the loop-like invaginations of the epithelium as glands, then we may admit the existence of Stieda's tubular glands ; then, also, we might with equal justice call the epithelial furrows glandular organs ; if not, then the invaginations are just as little to be regarded as glands as the furrows themselves, whose complete analogue they represent. Jacobson[1] and, quite recently, Ræhlmann[2] are also of this opinion. We must show that longitudinal as well as transverse sections of the conjunctiva show round sections of such blind sacs, which may be mistaken for the lumens of glands. Mandelstamm[3] describes tubular glands which run more or less parallel to the surface of the conjunctiva. But are these not transverse instead of longitudinal sections ?

The histological structure of Sæmisch's trachoma granule (Græfe and Sæmisch's " Handbuch d. Augenheilkunde," Bd. iv., fig. 2, p. 31) is not distinguishable from proliferated papillæ, and there is no justification for designating these structures as having specific qualities. The greater the proliferation of the papillæ the greater will be the number of these so-called trachoma glands. Now in trachoma these glands, originally running vertically to the surface of the conjunctiva, may, in consequence of the swelling of the parenchyma of the mucous membrane, be pushed out of their original position and extend diagonally to the surface ; then, of course, transverse sections would show round sections of these loops or pouches. The analogue of this condition is found in the intestine. Ziegler[4] has shown that in the mucosa of the intestine the glands of Lieber-

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[1] Jacobson, Jun : *Græfe's Archiv f. Oph.*, Bd. xxv., 2, p. 139.

[2] E. Ræhlmann : *Græfe's Archiv f. Oph.*, Bd. xxix., 2, p. 113.

[3] Mandelstamm : *Græfe's Arch. f. Oph.*, Bd. xv.

[4] Ziegler : " Lehrbuch der path. Anatomie," Jena, 1881, pp. 652–655. See also Cornil and Ranvier : " Manual d' Histologie Pathologique," vol. iii., p. 828.

kühn, in the neighborhood of Peyer's patches, which run perpendicularly to the surface of the mucous membrane, in consequence of infiltration and swelling of the follicles in abdominal typhus are forced out of place, so that in vertical sections of the membrane round sections of the glands are furnished. Now, if these so-called trachoma glands are the analogues of the epithelial fissures and invaginations which occur in the normal condition, then they cannot be pathological new-formations. Alongside of these invaginations, in trachoma, numerous actual interspaces sometimes occur in the conjunctiva, which are new-formations in the sense that by the swelling of the tissue the conjunctiva is folded in places. Now if ulceration of the apposed surfaces of the folds of the conjunctiva take place and in places grow together (fig. 4), the furrow may become constricted, and the development of partly communicating blind sacs give rise to the formation of cysts,[1] and in consequence of this ulcerative process the walls of the interspaces partly or wholly grow together, and in that way gland-like divisions occur in them (Mandelstamm). This tendency to cyst-formation is evidently accidental and not characteristic of the trachoma process; they are said to occur also in chronic blennorrhœa (Jacobson, Jun.). They nearly always occur at a late period of the disease. These cysts contain, so long as the epithelium is preserved, soft, tallow-like masses in which are epithelial cells, the latter being in places hyaline. Later the cysts contain a yellowish-white, pus-like fluid with few cellular elements, and of which they are readily emptied. This is a degenerative process; but sometimes a diametrically opposite condition obtains—*i. e.*, proliferation of the conjunctival epithelium; not a general uniform increase of the epithelial cells, but a localized proliferation,

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[1] Iwanoff: Bericht der Heidelberger oph. Gesellschaft, 1878, p. 18, figs. 3 and 4.

giving rise to inequality of the surface.   In regard to the proliferation of the epithelium inward into the tissue of the conjunctiva, which is said to be more frequent than the opposite condition just referred to, although I have seen it I can add nothing new from personal observation. Ræhlmann[1] refers this condition to ancient cases of trachoma in the regenerative stage, where defects have occurred in the surface of the conjunctiva, and these defects occur principally at those places where follicles have ruptured; he, therefore, refers those solid epithelial ingrowths to regenerative proliferation of the epithelium to cover loss of substance after ulceration and excoriation.

So far we have sketched follicular and trachomatous conjunctivitis as two conditions, but we have mainly tried to disprove certain propositions without indicating the true pathogenesis of either disease.

Actual trachoma bodies certainly do in rare instances disappear spontaneously, and such cases have undoubtedly been responsible for the confusion which exists in regard to trachoma, and have given rise to the assertion that special forms of the disease exist.   But we should never end if we attempted to enumerate all the classifications which have been offered regarding granular conjunctivitis.

Sæmisch,[2] de Wecker,[3] and Preuss[4] consider trachoma-granulations as new-formations of a specific character, completely distinguishable on the one hand from true follicles, and on the other hand from papillary proliferations. Sæmisch considers the closed follicle as the pathological basis of " follicular catarrh." This is contrary to the experience of others, and an examination of the sections which I have obtained from conjunctivas in different stages of the

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[1] Ræhlmann : *Græfe's Archiv f. Oph.*, Bd. xxix., 2, p. 130.
[2] Græfe u. Sæmisch : " Handbuch der Augenheilkunde," Bd. iv.
[3] De Wecker : Text-book.
[4] Preuss : *Berliner klin. Wochenschrift*, 1869, Nos. 42, 43.

trachoma process, would contradict this proposition, and show that the "*follicile*" *is a very faithful attendant of trachoma.* That which has been denominated follicle, both in trachoma and follicular catarrh, is an accumulation of lymphoid cells of a circular or oval, and sometimes irregular shape. We have found these structures a pretty constant product in all forms of trachoma. This means that the character of the conjunctival affection, in the one case innocent, in the other serious, does not depend upon the presence of the follicles. Just as the diffuse infiltration of lymph cells in the tissue of the conjunctiva is only a product of inflammation, so is the follicle due to a like cause; its further history depending on various accessory circumstances, such as intensity and duration of the inflammation and the power of resistance in the tissues, the presence or absence of the follicle having nothing in itself characteristic of the disease. The arrangement of the lymphoid cells into clusters or follicles is a peculiar circumstance for which I know no satisfactory explanation. There is certainly no justification upon pathologico-anatomical grounds for distinguishing trachoma from follicular catarrh, and inasmuch as the majority of histologists agree that the follicle does not exist in the normal human conjunctiva, and as we have seen that no other anatomical condition is characteristic of trachoma except the follicle, we may, with good reason, define trachoma as a follicular disease.

The mixed trachoma of Stellwag is nothing more than a transition stage; or, as Schmidt-Rimpler[1] says, there exists only a quantitative, no qualitative, difference between follicular catarrh and trachoma.

It is difficult to reconcile the statement of Sæmisch in regard to the differentiation of the follicle from papillæ. This writer[2] furnishes a drawing of the "trachoma granule,"

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[1] Schmidt-Rimpler: *Nagel's Jahresbericht* Bd. vi., p. 211.
[2] Græfe und Sæmisch : " Handbuch der Augenheilkunde," Bd. iv., 2, p. 42.

and says that it is distinguished from papillæ in that it is a prominence of the mucous membrane, the interior of which is infiltrated with lymphoid cells; and from the follicle in that the latter is a roundish, semicircular structure, sharply circumscribed beneath the level of the conjunctiva. But he says, although the histological elements of the "granulation" and those of the swelled papillæ may be classed alike, not alone are form and appearance of the prominences in each case distinguishable the one from the other, but that the structure—the formation—of the granulation is essentially different from that of the papillæ. *L. c.* p. 45. There is, then, according to Sæmisch, a specific trachoma granule. I have never been able to confirm this proposition. In those cases in which portions of the conjunctiva were excised, which microscopically presented the appearance described by Sæmisch, of a round, grayish-red prominence, especially in the region of the posterior fold of the conjunctiva, they were found microscopically to consist either of proliferated papillæ which had coalesced or rather grown together, or of follicles.

There is a vesicular formation on the conjunctival surface which some writers have described as "vesicular granulations," and which has frequently been stated to be premonitory of granular conjunctivitis. Sæmisch aptly describes them as "small, semi-globular, nearly transparent prominences, not quite half the size of a pin's head, which occur most frequently on the conjunctiva palpebræ inferioris, between the posterior fold of the conjunctiva and the free border of the lid, and principally toward the external commissure." These formations are also frequently seen beneath the upper lid, near the external commissure, as an isolated cluster of small vesicular bodies surrounded by swelled, red conjunctiva, the rest of the conjunctiva being apparently normal or but slightly hyperæmic, and we know

that this condition may remain unchanged for a very long time, and without giving rise to any marked symptoms. These vesicles are generally produced by ectasia or a lifting up by a watery fluid of the epithelium, and sometimes the superficial layers of the conjunctiva. If you puncture these bodies with a needle, a clear fluid escapes, and the vesicle collapses. Sœlberg Wells speaks of a vesicular condition which he describes as having escaped the attention of other authors, and which he says is premonitory of " granular ophthalmia," and that it is due to the enlargement of " pre-existing lymph follicles in the conjunctiva." But this view was held by Burkard Eble already in 1820, and Helling,[1] Gonzée,[2] and Decondé.[3] However, these authors employed the term vesicular granulations to designate " trachoma granules," and they believed that the latter developed from the vessels. If one or more of these bodies, including a portion of the surrounding swelled conjunctiva, are removed with the scissors and hardened in alcohol, you will see in horizontal sections that these bodies are surrounded by a uniformly thick layer of epithelium with small invaginations here and there. Beneath the epithelial layer is proliferated, partly selerosed connective-tissue, permeated by numerous lacunæ-like lymph-spaces, which are lined with endothelium, and contain a finely granular amorphous mass (Ræhlmann).

Under normal conditions, the tarsal conjunctiva is attached to the tarsus only by a loose connective tissue. In chronic cases, the follicles are collected in heaps, and as some of these deposits disappear new ones develop, and it is just in the chronic cases that connective-tissue formations and sclerosis attain to an extraordinary degree. In

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[1] Helling : " Praktisches Handbuch der Augenheilk.," Berlin, 1881. Bd. ii., p. 117.

[2] Gonzée : " De l' ophthalmie qui regne dans l' armée belge." Bruxelles et Paris, 1842, p. 219.

[3] Decondé : " Memoire sur differentes questions qui se rallachant à l' ophthalmie de l' armée." Annales de la Société de Méd, de Gand, Juin, 1840, p. 131.

consequence of progressive follicular formation and associ-
ated disappearance of follicles, the adenoid tissue of the
conjunctiva is thickened, and later on destroyed; on the
other hand, the sclerosis process converts the loose con-
nective tissue which separates the adenoid layer from the
so-called cartilage of the lid, into a mass of dense connective
tissue which cannot be distinguished from the fibrous frame-
work of the lid. This young connective tissue undergoes
sclerosis and cicatricial retraction. The loose tissue between
the cartilage and conjunctiva being obliterated, or rather,
incorporated with the cartilage, it is easy to understand
what would be the mechanical effect of cicatricial contrac-
tion of this tissue. This retraction is generally independent
of any change in the fibrous framework (cartilage); but of
course the latter may, in long-continued cases, also be
involved. As a rule, the cicatricial changes begin in the
deeper parts of the conjunctiva, and pass thence outward.

The margins of the lids are nearly always involved in the
inflammatory process, and an explanation for this indepen-
dence on the one hand and constant implication on the other
is furnished by the condition of the vascular supply in the
conjunctiva, which, excepting a few anastomotic branches,
is separated from that of the cartilage; but the conjunc-
tival vessels are very intimately connected with the border
of the lids.[1] In addition to the conjunctiva palpebrarum,
that of the bulbus and the cornea is also involved at some
period of the disease; in the latter instance constituting the
condition known as pannus corneæ. But in the case of the
cornea it is principally the epithelial layer that is involved.
This condition is well shown in a drawing by Irvanoff,
in Græfe and Sæmisch's vol. iv., p. 188. Here we see a
subepithelial infiltration consisting of small round cells,
the cornea proper not generally being involved.

[1] See diagrams in Langer's paper: " Ueber die Blutgefässe im Augenlide."
Med. Jahrbücher, 1878, Heft. 3, p. 333.

· TREATMENT.

The presence of granulations in the conjunctiva does not furnish an immunity against other forms of conjunctivitis, and when they accompany the granulations the condition is intensified, because the secretions which in those cases in which the inflammation is absent or slight are eliminated in a great measure by the tears, lodge between the granulations, especially in the cul-de-sacs, and by decomposition furnish another source of irritation and a means of contagion. It is just here that solutions of corrosive sublimate are useful, on account of the astringent properties of the drug, for the mechanical effect of removing the secretions. Of course this decomposition implies the presence of bacterial life, because we know that without them decomposition does not take place.

If we examine the methods which have been employed for treating granular conjunctivitis from the earliest time (the disease was known to Hippocrates,) to the present day, we shall see that the efforts of all have been directed toward destroying the granulations, by a multitude of means, which collectively constitute a chapter as confusing as it is elaborate. But it would be an idle waste of time to enumerate all the substances which have been employed to combat this obstinate disease. The diversity of the methods of treatment originated from the different indications which were thought to best meet the requirements of the disease. Let me first speak briefly of caustics. Some surgeons have proposed to meet the indications by having recourse to caustics. To destroy the granulations may be correct enough, but the caustic also destroys parts we do not desire to influence by it at all; *i. e.*, when you destroy the granulations you likewise destroy the conjunctiva, and you can readily anticipate the mischief which will result when the process of repair after this cauterization is effected. There

was a time not so very long ago when the solid stick of argent. nit. was quite freely employed in trachoma, and even to-day we occasionally hear of its being used. Caustics may have yielded good results in a few instances when employed with intelligence and prudence, but in an overwhelmingly large majority of the cases in which it was employed for granular conjunctivitis it occasioned a great deal of suffering to the patient, and he was not recompensed by a good result, and pretty generally another condition was substituted which was even more serious than the original trouble.

If you examine sections of trachomatous conjunctivas, you will see that the tissue of the granulations is lymphoid in its nature, and also that the vascularity is generally very pronounced, and it is to the presence of these vessels that we may justly refer the production of the new-formed cells. Astringents, we know, diminish the diameter of capillary vessels. But it is a great desideratum that the action of the astringent should be *continuous*, otherwise we may produce an opposite effect. The great clinician Trousseau said : " When it is desired to bring about atrophy with astringents, their use must be energetic and sustained."

With the announcement of the discovery of micro-organisms in trachoma tissue, came, of course, the suggestion for the employment of parasiticides ; and corrosive sublimate, the recognized most effective among these, was naturally seized upon. Sattler employed it in blennorrhœa of the conjunctiva in solution $\frac{1}{20,000}$, but I am not aware that he used it in trachoma. Quite soon after the announcement of Sattler's discovery, I used corrosive sublimate in trachoma as a parasiticide ; but as I investigated the subject for myself, and came to the conclusion that there were no bacteria in the trachoma tissue, I still continued to use it, but mainly for its astringent and mechanical effect. The

astringency of corrosive sublimate is indeed very marked when employed in a one-fourth-per-cent. solution. From personal experience in the use of corrosive sublimate in trachoma I cannot furnish any flattering results; but its solutions remain clean and free from micro-organisms, and for this reason are commendable, especially when they have to be used by a certain class of patients who are not over careful with regard to cleanliness. But you will have to watch your patients, because the corrosive sublimate is very irritating to some eyes, even in very weak solutions of $\frac{1}{10,000}$ or $\frac{1}{20,000}$. I think the best way of applying the solution to the conjunctiva is by means of the spray. A few years ago I had the privilege of introducing this means of making applications in conjunctival affections, but there is one point, to which I made no allusion in my paper,[1] and that is, the mechanical effect of the spray in removing secretions from the conjunctiva, which, as I have stated above, is of especial importance in granular conjunctivitis, because of the collection of secretion between the granules and in the cul-de-sac, and further, because patients, when they wash their eyes, do so generally with their eyelids closed.

The sulphate of copper in crystal is perhaps the most popular and generally employed of the astringents in granular conjunctivitis. But the perfunctory way in which it is so universally used in trachoma often aggravates the disease; however, copper, argentum nitr., and subacetate of lead deserve a conspicuous place in the treatment of trachoma, but they require to be used with a great deal of intelligence. Respecting the subacetate of lead, it may be urged that it forms deposits on the conjunctiva and cornea; nevertheless, if employed in a rational manner, and in conformity to the object you wish to attain, the result will be found to be often better than those obtained from copper;

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[1] ARCHIVES OF MEDICINE, 1879.

but, on account of the possibility of its forming deposits on the cornea, it is not a remedy which should be given to patients to use for themselves. Of course, I appreciate the fact that many cases must run the gauntlet of a number of remedies.

There is another method of treatment which has undeservedly, I think, been neglected. I refer to *excision* of the cul-de-sac of the conjunctiva in granular conjunctivitis, first practised, I believe, by Richet, and subsequently and more enthusiastically advocated by Galezowski. I have done this operation in eleven cases. But it should be resorted to, I believe, only as a *dernier ressort*. In three of these cases, operated on about five years ago, in which there was pronounced pannus corneæ, the latter condition was favorably influenced by the excision, and to this day no deformity exists—*i. e.*, no entropion.

The excision is done in the following manner : Seize the superior or inferior cul-de-sac, as the case may be, draw it away from the tarsus, and excise the whole length of the cul-de-sac. Thus you *detach the cul-de-sac entire, without touching the tarsus or the conjunctiva bulbi. Of course, you should not excise the granulations of the tarsus;* the cicatrization which would follow the excision of the tarsal conjunctiva would do infinitely more mischief than the granulations themselves; and, then, the granulations on this part are much less rebellious to treatment than are those in the cul-de-sac; indeed, in the few cases in which jequirity did not produce a perfect result, the granulations had disappeared entirely from the tarsal portion, but remained in the cul-de-sac. After the excision you will generally have to employ astringents.

I come now to consider *jequirity ophthalmia*. In the number of the *Annales d'Oculistique* for July and August, 1882, Dr. de Wecker of Paris, directed attention to the cura-

tive virtues, in trachoma, of the Brazilian plant, jequirity. So far as I have been able to ascertain, jequirity is a local term used by the natives of Brazil to designate its longer botanical name *abrus pecatorius.* It is a twining woody shrub indigenous to India, but now found in all tropical countries. The plant is mentioned in the Sanskrit medical writings of Susrata, whence we may infer that it has long been employed in India.[1] It is also known as Indian liquorice, Réglisse d'Amerique, Radix abri, and Liane à réglisse. It was introduced into the Bengal pharmacopœia of 1844, and into the pharmacopœia of India of 1868. The seeds, of the size of a small pea, weigh about $2\frac{3}{16}$ gm.; they are glossy, very hard and tough, and of a beautiful bright-red color with a black spot. In India they are used for making chaplets and necklaces, and we occasionally see them used in our own country for ornamental purposes. When the infusion of the root is mixed with an alkaline solution of tartrate of cupper, red cuprous oxide is deposited after a short time.

Although it has long since been settled by experimental proof that we do not possess an agent which exercises an action on the trachoma bodies similar to blennorrhoic pus, yet we have found in jequirity one which, by its being controllable, will receive a much wider application.

To de Wecker belongs the great credit of introducing jequirity into ophthalmic practice. The claim of Moura of Brazil was too late to merit more than the remark which de Wecker made respecting that writer's communication— *i. e.,* that his (de Wecker's) presentation of jequirity to the Academy of Sciences, in August, 1882, had exercised a certain emmenagogue effect on his Brazilian confrère.

The directions for preparing the infusion, as given by de Wecker, were to pulverize thirty-two of the berries (about

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[1] Pharmacographia : By Friedrich A. Flückiger and Danial Hanbury.

three grammes by weight) and macerate the product for twenty-four hours in 500 grammes of cold water ; then add 500 grammes of hot water ; filter immediately after the liquid cools. The patient is to bathe the eyes with this lotion three times in the day. If the resulting inflammation is severe this will be enough ; otherwise the bathing must be repeated on the second day, and if necessary even on the third day, the same liquid being used. The patient must remain in a dark room for fifteen days. However, it is not necessary to adhere to these directions. The infusion may be prepared weaker and without the aid of boiling water, and the patient need not remain in a dark room while the applications are being made.

I prepare the infusion as follows : I have generally attended to this myself. The seeds are first deprived of their red husks by cracking them in a clean linen bag. I then grind them into a powder (not necessarily fine) in a small pepper-mill ; then add cold distilled water or glycerine and set aside for five or six hours ; then filter, and the preparation is ready for use. The watery preparation should be fresh, but the glycerine preparation, if kept in a closed vessel, will be safe and useful after several days, or even weeks. The strength of the infusion should vary according to the nature of the case in which it is employed—1 %, 2 %, 3 % ; the 5 % infusion being reserved for those cases in which the changes in the conjunctiva and cornea are advanced, and in which the conjunctiva is dry and degenerative changes have already taken place. This strength may be employed in other cases in which the changes are less pronounced, but we should always proceed with caution. I apply the infusion to the inverted eyelid by means of absorbent cotton wrapped about the end of a match and dipped into the infusion, and repeat it two or three times during the day and then await results. *When the cornea is not involved I*

make a single application of a fresh one-per-cent. infusion and wait thirty-six hours before repeating it or a stronger (two-per-cent.) infusion.

De Wecker [1] advises to generalize the application of the infusion in all the cases of granulations, even the less intense, but he condemns its use for the removal of papillary hypertrophy, such as is produced by chronic purulent ophthalmia; and *where suppuration already exists artificial suppuration must not be superadded.* But there is one circumstance to which de Wecker alludes which should be corrected—*i. e.,* that the cornea is not exposed to any risk during the evolution of jequirity ophthalmia. Abadie [2] states that he saw both corneæ completely destroyed by jequirity ophthalmia. However, I believe that *we can control the action of the infusion by regulating its strength, by limiting the number of applicacions, and by taking advantage of the fact that there is a period of incubation, and this I would particularly commend in those cases of trachoma in which the cornea is not affected,* De Wecker stated that purulence was an essential feature of the ophthalmia, but the sooner this error is eradicated the better, because it may lead to the most disastrous results if we persist in our applications of the infusion until purulency is produced. *Neither purulence nor membrane is an essential feature of the inflammation.* In the majority of my fifty-seven cases there was no membrane, and pure purulence existed only in a very few instances.

Of my fifty-seven cases only three were without result; seventeen were partial cures—*i. e.,* the granulations disappeared from the tarsal conjunctiva, but some remained in the cul-de-sac; in two of these cases, subsequent treatment with argent. nit. removed the granulations. Only in one instance was the use of the infusion attended with an un-

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[1] De Wecker: *Annales d' Oculistique*, Mai et Juin, 1883, p. 224.
[2] Abadie: *Annales d' Oculistique*, Mai et Juin, 1882.

favorable result. The patient, a man, æt. twenty-eight, had had trachoma for three years ; both corneæ were clear. There was some mucous discharge in the morning on awaking. I treated him during two months, first with argent. nit., and then with copper, but there was no appreciable change. Flushed with the success I had had with my first jequirity case, I determined to try the remedy in this second one. I first applied a 2 % infusion which was ten days old, but it gave rise to very slight reaction, and so I ventured to try a fresh 5 % infusion. This was applied on a Saturday morning at 7 o'clock. The next day at noon the patient's eyelids were œdematous and there was considerable serous chemosis; the discharge from the eye was serous; and the patient complained of severe pain in his forehead and upper incisor teeth ; there was also a profuse serous discharge from the nose, and the eyelids were tender to the touch. No further application of the infusion was made. On the third day at 9 A. M. there was a slight thick and stringy discharge of a straw color from both eyes ; the lids were swollen and very firm ; the chemosis was very pronounced and of a dense red color. At 6 P. M. of the same day there was a patch of diphtheritie membrane about $\frac{1}{4}$ in. square on the ocular conjunctiva near the periphery of the cornea, and the latter was infiltrated. The diphtheritic condition alarmed me, and I at once proceeded to do what I could to avert the disaster which I considered imminent. There was an extensive ulceration of both corneæ ; this healed kindly in the left eye, but there remained considerable loss of tissue in the right cornea. Neither corneæ perforated. All the granulations disappeared from the palpebral conjunctiva of both eyes, but many still remained in the cul-de-sac. The patient was subsequently treated with argent. nit., and after five months he passed from treatment without any true granulations. His vision was R. E. $\frac{20}{50}$, L. E. $\frac{20}{20}$. This was the

second case of trachoma which I had treated with jequirity, and it convinced me of two things—*i. e.*, first, that *the infusion should be fresh; second, that* 5 % *is entirely too strong to use in any case in which the cornea is not involved.* A distinguished ophthalmologist of this city had a still worse experience with a 5 % infusion in a case in which the cornea was clear—the latter having perforated, healing taking place with adherent leucoma. However, it would be unfair to use these two cases as evidence against the usefulness of jequirity in trachoma even where the cornea is not involved, because I am convinced that the danger may be avoided by following the directions which I shall speak of further on. In the remaining thirty-seven cases the result was complete cure. I mean by this that the granulations disappeared from every part of the conjunctiva; that the pannus, which was pronounced in the majority of the thirty-seven cases, disappeared; and the sight was improved in the majority of the cases from an ability to perceive light only, to vision $\frac{20}{100}$, and in one of these extreme cases the vision was improved after two months to $\frac{20}{70}$, and there is reason to believe that the vision may become even better in time.

These results show that we have in jequirity a remedial agent of decided value, and although it does not always immediately destroy all the granulations, it diminishes, in a very pronounced degree, the pain and photophobia, and has a marked influence in clearing the cornea.

From October, 1882, to January, 1884, Coppez[1] employed jequirity in 164 cases, of which 140 had granulations often complicated with inveterate pannus, which had resisted other means of treatment. Jequirity was especially successful in old trachoma with rebellious pannus—78 cures among 140 cases. The accidents were augmentation of the

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[1] M. Coppez (Brussels) : *Recueil d' Ophthalmologie*, Février, 1884, p. 67.

opacity of the cornea, or ulceration was even produced of this part ; but Coppez states that renewed application of the infusion always dispersed the corneal trouble. He had seen hypopyon ectropium of inferior lid, erythema of the face and lids ; and, in the case of a young girl with trachoma, pannus and ozœna,—all three troubles disappeared simultaneously.

There is a point of very great importance in the use of jequirity which is in striking contrast to that of inoculation with blennorrhoic pus, and that is that with the latter you can inoculate only in case both eyes are affected, because of the danger of communicating the contagion to the fellow eye. *The discharge in pure jequirity ophthalmia does not possess the property of contagion.* I have again and again tried to reproduce the inflammation in jequirity ophthalmia by inoculating (rabbits) the good eye with secretion taken from the eye under jequirity, but always with negative results. Nevertheless we should exercise a great deal of caution when using this remedy in case one eye only is affected, and bear in mind the possibility of the infusion taking on infectious qualities through long exposure to the air. The infusion should on this account be prepared fresh, and *all infusions several days or weeks old should be discarded.* I mention this circumstance because I observed that, in one instance in which the infusion was prepared in the ordinary way and kept on the table for several days, it gave rise when applied to a rabbit's eye to a true infectious disease. But I am not sure that this infectious quality may not have been acquired, during my absence from the laboratory, from contamination of the infusion in some other way than mere contact with the air. However, I consider this fact a strong argument for using *none but fresh infusions.*

The infusion should be applied in such a way that its action will be confined as much as can be to the parts you

wish to influence, for in this manner it does seem that you will be better able to keep it under control.

Menacho [1] (assistant to de Wecker) records three hundred cases which were subject to this treatment during the second half of the year 1883 in Dr. de Wecker's clinique. They advise its use in the following class of cases : 1. Forms of torpid ulcers of cornea, facette ulcers, and principally cicatricial keratitis. 2. Strumous pannus tenuis. 3. In conditions consecutive to parenchymatous keratitis, infiltrations, and sclerosis.

Parisotti and Galezowski [2] used jequirity in ten cases, but not with the best results. Deneffe [3] declared that jequirity is inoperative in pannus ; but this statement is completely at variance with facts, for it is just in such cases that jequirity furnishes such brilliant results. There are others who have been unsuccessful with jequirity, but the evidence furnished by de Wecker, Coppez, Terson, Vacher, Gruening, and numerous others, encourages us to expect better things from it.

After a single application of jequirity infusion to the eye, there is no perceptible change for several hours ; the phenomena develop at first slowly, and after four or five hours there may be only a slight increase in the flow of tears ; after eight hours the hyperæmia may be only slightly pronounced, but after twenty-four to thirty-six hours the eye presents the picture of a pretty intense inflammation. The lids are swelled and matted together, and, in the case of a human being, very painful to the touch. After forty-eight hours the tarsal conjunctiva is generally covered with a more or less adherent whitish or grayish-yellow membrane. The secretion is generally muco-purulent ; the palpebral con-

[1] Menacho : Sur les indications thérapeutiques du jequirity dans les affections de la cornée, *Revue d' Oculistique*, Jan., 1884, p. 14.

[2] Parisotti and Galezowski : *Recueil d' Ophthalmologie*, Jan., 1884, p. 31.

[3] Deneffe : *Recueil d' Ophthalmologie*, May, 1883, p. 248.

junctiva is also greatly swelled and firm, and the ocular
conjunctiva is frequently, especially in the delicate rabbit,
chemotic, and the cornea is nearly always involved in these
animals. On the third or fourth day the inflammation
begins to recede, and after three or four weeks there
remains perhaps a slight opacity of the cornea and some
injection of the conjunctiva; but this condition will vary
according to the strength of the infusion and the suscepti-
bility of the animal. This is what generally obtains after a
single application of the infusion in the rabbit.

THE NATURE OF JEQUIRITY OPHTHALMIA.

At Dr. de Wecker's request, Dr. Sattler of Erlangen un-
dertook a series of experiments for the purpose of deter-
mining the nature of jequirity-ophthalmia, and the results
of his investigations have been published in the *Klinische
Monatsblätter f. Augenheilkunde* for June, 1883, p. 259. Pro-
fessor Hilger and Dr. Giesmann tried to isolate, chemically,
the active principle of the berry, but neither of these gen-
tlemen succeeded in doing so, and so far similar attempts
have been equally unsuccessful. Sattler concluded from
his experiments that the active agent in the infusion was a
particular bacillus, derived from the atmosphere, and that
although harmless in itself, its spores developed in the in-
fusion new physiological qualities (*l. c.*, p. 224); in other
words, that they were metamorphized from an innocent into
a pathogenic bacterium. But such a transformation has
not yet been proven, nor does Sattler furnish a demonstra-
tion of it. However, Sattler maintained that jequirity
ophthalmia was an infectious disease. In order to solve
this problem it was necessary to show that a distinct bac-
terium existed in the infusion, which constantly returns in
all infusions of the berry, and it should be found again in
the tissues which it infects, and also in the secretions fur-
nished by the latter. It must further be shown that the

infusion is without effect when the bacterium is removed or rendered inactive. Finally, the particular bacterium of the infusion should be transferred to a suitable culture medium, and rendered pure by a series of generations, and when the pure culture is applied to the conjunctiva, it should reproduce the disease. Sattler claims that his experiments justify him in answering these questions affirmatively. He maintains that the infusion contains a great number of bacilli, 0.58 mm. thick, and about 2.5 mm. to 4.5 mm. long, at times motionless, at other times moving about in a very lively way, with oscillatory or rotatory movements. These bacilli are very readily demonstrated in all fresh jequirity infusions, and there are generally other varieties of bacteria; but the bacilli of the size referred to above, are always present in greater numbers than any other forms. We must agree with Sattler so far as the presence of the bacillus is concerned, but in another direction my experiments have led me to views diametrically opposed to those of Sattler—*i. e., jequirity ophthalmia is not a bacterial disease*, and that instead of bacteria being the active agents, they, on the contrary, by giving rise to putrefactive changes, actually destroy the peculiar properties of the infusion. Let us examine briefly the evidence which contradicts Professor Sattler's view respecting the nature of jequirity ophthalmia.

If the inflammation produced by a jequirity infusion be referable to the presence of bacteria in or their action upon the infusion, then when the infusion is prepared in such a manner as to exclude all bacteria, such an infusion should be inoperative. To decide this question, I proceeded as follows: A number of jequirity berries, previously weighed, were placed for twenty minutes in a 0.1 % solution of corrosive sublimate; then they were dried with a cloth which had been kept in the sterilizer at 180° C. for two hours; the red husks were

then removed with the aid of strong pincers previously
sterilized by heat, two pincers being used in order to avoid
touching the berry with the fingers; the berry was now
crushed, or only squeezed, and dropped at once into a
sterilized flask containing cold water or glycerine which had
been previously subjected to 100° C. in the sterilizer during
two hours; the flasks were then stopped with sterilized ab-
sorbent cotton-wool, and laid aside, at the temperature of
the room, about 68° Fahrenheit, for use after from four to
thirty hours.   The infusions were in strength ½ %, 1 %, 2 %,
3 %, and 4 %.   At the time of use, the selected infusion was
examined microscopically for bacteria, and gelatine bouillon
(prepared according to Koch's directions) was inoculated
with the same infusion, in order to make the test complete.
These infusions gave rise to the characteristic inflammation
in every instance when applied to the conjunctiva of rabbits
and patients who had trachoma, *a distinct period of incuba-
tion being observed in every instance;* but microscopic ex-
amination of the infusion did not reveal any bacteria, and
the gelatine bouillon remained free from bacteria, although
it was kept in an incubator at a temperature varying from
34° C. to 36° C.   These experiments were repeated a great
many times, and in every instance the most rigid precautions
were observed to exclude all bacteria.   According to Prof.
Sattler's proposition, a pure culture of the particular bacillus
found in the infusion should reproduce the characteristic
inflammation when applied to the conjunctiva of animals
which we know to be susceptible to the contagion ; but this
in my hands failed in every instance.   Nor was I ever able to
demonstrate the presence of the bacilli in the secretion or
in the membrane formed on the conjunctiva, or in the con-
junctival tissue itself : Neiser,[1] Salomonson,[2] and Klein,[3]

[1] A. Neiser (Breslau), *Fortschritte der Medicin*, No. 3, 1884, page 73.

[2] Dr. C. S. Salomonson, stud. med.   J. Christmas Dirckinck-Holmfeld in
Kopenhagen : *Ibid.*, No. 3, page 78.

[3] Klein (London), *Centralblatt für prakt. Augenheilk*, 1884, page 55.

have had similar experience. In some instances, the secretions from the inflamed eyes under jequirity did contain micrococci, but this is a common circumstance, as they are also often found under normal conditions.

I did not rely upon filtration for excluding bacteria from the infusion, because if the specific action of the latter were due to the presence of bacteria or to a particular bacillus, or to the latter giving rise to certain changes (chemical) in the infusion, then filtration of the liquid—*i. e.*, through clay —would, if the infusion were still operative, not dispose of the proposition that bacteria were the immediate cause of the inflammation of the eye to which it had given rise, because the infusion may have acquired noxious properties through the previous vital activity of the organism. Here I may mention by way of analogy, that the resulting infection, after inoculation of rabbits with animal tissues in various stages of putrefaction, is just as certain if the putrid substance be previously boiled and thereby deprived of living organisms, since putrefaction must be regarded, in the present state of our knowledge, as impossible without the presence of bacteria; and it is evident that in such cases sepsis was due, directly or indirectly, to the action of bacteria, because even the boiled substances used by Panum and Rosenberger, and the crystalline substance obtained by Bergmann and Schmiedeberg from putrefying beer, and which they termed sulphate of sepsin, had acquired their septic properties through putrefaction—*i. e.*, through the action of bacteria.

However, although the bacterial agency be opposed by very substantial proof, there is no evidence which contradicts the probability that the active principle of the infusion is a *vegetable substance*, and this agent is soluble in water and glycerine, and is destroyed by boiling.

Subcutaneous injections of an infusion of jequirity in rabbits kill these animals, and the death may be quite independent of any bacterial agency. I have convinced myself of this fact by repeated experiments with the infusion prepared under precautions. I should not speak of this circumstance here, but Cornil and Berlioz [1] have recently published the accounts of experiments upon rabbits and other animals, in which they injected a decoction of the berry, subcutaneously; death was produced within thirty-six to sixty hours. At the site of the injection there was an inflammatory œdema of the skin; also slight peritonitis, ecchymoses of mucosa of stomach, and tumefaction of Peyer's patches in the intestines, bacilli being found in large numbers in the blood of the heart, and all the vessels containing a great quantity of live bacteria; furnishing, in brief, the characteristic phenomena of an infectious disease. These gentlemen concluded that the bacteria of jequirity are the sole active principle. But the jequirity infusion generally contains such a variety of bacteria, that the experiments of Cornil and Berlioz only show what may be produced with many other vegetable infusions which are favorable media for certain bacteria. One of their rabbits recovered from a phlegmon and local gangrene determined by subcutaneous injection of a small dose of the decoction; they assert that it acquired immunity against subsequent infection. But in respect to this proposition we may recall Koch's reply to Pasteur's theory respecting immunity against infectious diseases, namely, that although some of the infectious diseases occur in the same animal but once, as a rule, yet no immunity is secured from others by the first attack, and adduces erysipelas, gonorrhœa, intermittent and recurrent fever, as examples familiar to all, and that anthrax not infrequently occurs twice in the same individual.

[1] Cornil et Berlioz: Sur l'empoisonnement par le jequirity. Comptes Rendus Heb. des Séances de l'Académie des Sciences, 1883, No. 12, p. 679, and No. 15, p. 806.

I desire to express my indebtedness to Dr. T. Mitchell Prudden, Director of the Pathological Laboratory of the Alumni Association of the College of Physicians and Surgeons, New York, for many kindnesses and suggestions while conducting my experiments with jequirity and supposed bacterial trachomatous tissues.

A CONTRIBUTION OF CASES ILLUSTRATING CRANIO-CEREBRAL TOPOGRAPHY.*

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THE following cases I have had under my observation and care in Bellevue Hospital, and they illustrate, with accuracy and almost exactitude, the relationship existing between cranio-cerebral topography and symptoms. I will first read the histories of the cases I have to report, and cite a number of analogous cases, closing with a short dissertation on the relative modes of treatment, and especially the antiseptic method.

CASE I.—Michael D., aged twenty-six, Italian, a laborer, entered May 7, 1882. The history obtained was that four weeks ago he was struck over the right parietal bone with a stone. He fell senseless, but recovering went to bed very weak and with a severe headache. After ten days he resumed his work feeling as well as ever. On the fourth day after resuming his work he was obliged to cease on account of the gradual onset of paralysis involving the whole left side of the body. At first there were convulsive movements and occasional pain in both the upper and the lower extremities of the affected side. Complete flaccidity followed, and a sensation as though the parts were asleep. Coincident with these symptoms at their onset were the return of the headache over the seat of injury, and chills, now and then accompanied, as he thought, with fever. He was not delirious or comatose, and his functions were normal. On admission, the temperature in the right axilla was 99¼°; in the left, 101°. Pupils normal. He had

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* Read before the New York Neurological Society, June 3, '84.

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a convulsion when he started for the hospital, and one on the following day. The spasms were clonic in character, and affected the left side of the trunk, the upper and lower extremities of the left side, the sterno-mastoid, and the left half of the occipito-frontalis. During the attack the rectal temperature rose to $104\frac{1}{2}°$. He was apparently conscious during the convulsion, and certainly so immediately after it. May 9th he was operated upon by Dr. Keyes.
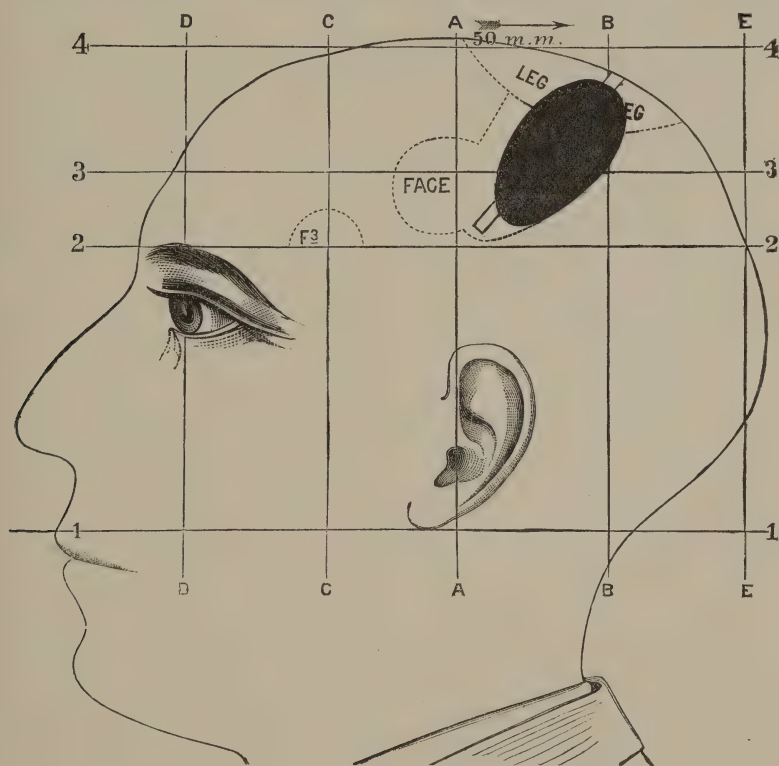
FIG. I.

A crucial incision was made over the seat of injury, the pericranium pushed aside, and the bone examined. It was found to be very much softened and highly vascular—bleeding profusely over its entire surface. Golt's trephine was applied so as to include the edge of the lesion. It was found that the degenerative process had penetrated to the inner table. The dura was hyperæmic. Exploration beneath the dura with a hypodermic syringe failed to find pus. No accurate cranial measurements were taken, but the

lesion was in the left parietal bone, and over the situation of the Rolandic line. It is approximately well indicated in the accompanying illustration ; see fig. 1. The patient had two convulsions, similar in their character to those already described, within thirty-six hours after the operation. After that time they did not recur. The hemiplegia gradually disappeared, and he left the hospital cured.

CASE 2.—Edward H., aged seventeen, a painter, entered June 20, 1883. He was injured a short time before by a brick falling from a building, striking him on the right side of the head. When admitted, patient was in no shock ; his pulse was full and strong, about eighty per minute. The pupils were equal and responded to light. He was conscious and answered questions intelligently, but seemed to be dazed and stupid. There was complete paralysis of the left upper extremity, partial paralysis of the left lower extremity, also a slight amount of left facial paralysis. Sensation normal. On the right side of the head there was an extensive lacerated wound of the scalp, at the bottom of which depressed bone could be easily felt. Operation was decided upon and performed by Dr. F. Lange. Extending the scalp wound toward the median line an elliptical area of bone was found depressed. From subsequent elaborate measurements made of the area after recovery I have abstracted the following brief report : " The bregma, as found by erecting on the alveolo-condyloid or base line a perpendicular passing through the external auditory meatus, was situated 19½ centimetres from the root of the nose. The anterior limit of the depression was 2½ centimetres caudad of the bregma ; the posterior limit, 7½ centimetres caudad of the bregma. As regards the lateral diameter, the median edge of the depression was 1 centimetre from the median line ; the lateral edge, 10 centimetres. From the tragus to the lateral border of the depression it was 12.7 centimetres. The depression was somewhat lozenge-shaped, 9 centimetres in its long axis, which was transverse, and 5 centimetres in its shorter axis, which was obliquely sagittal." In general terms the depression was, above and behind the ear, entirely clear caudad of the auriculo-bregmatic line. *Vide* fig. 2. The elliptical area of depressed bone was removed and the edges of the firm bone rendered smooth. A small laceration in the dura mater was discovered near the superior longitudinal sinus, but there was little hemorrhage from the wound. The remnants of the periosteum were drawn together, the slight hemorrhage controlled with ice, and

the flaps sutured with catgut, two small drainage tubes, being employed. Complete antiseptic precautions were observed in the operation and in the dressing, and primary union of the scalp was obtained. The wound healed under two dressings. At the end of a week he regained power in the lower extremity, and shortly after in the upper extremity and the face. I have him under observation still and he has at no time developed other symp-
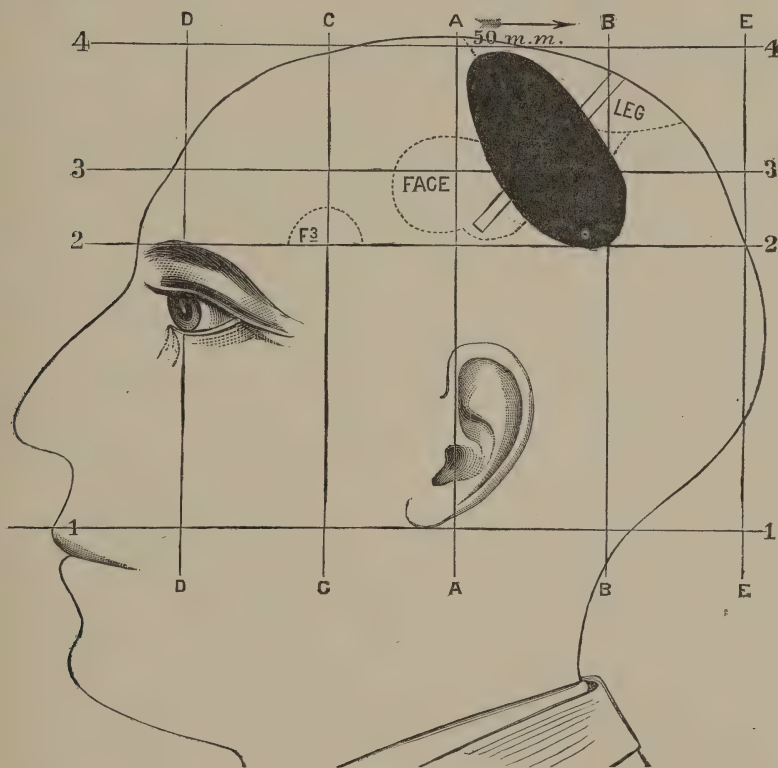
FIG. 2.

toms. No re-formation of bone occurred, but that was hardly to be expected considering the amount of bone removed.

CASE 3.—This case is interesting as illustrating pressure due to pus instead of bone, it being a case of compound depressed fracture without symptoms, which was trephined, followed by meningitis with the formation of pus, especially over the motor area, with the development of symptoms, first, of slight pressure causing irritation of the cortex, later, pressure sufficient to cause com-

plete paresis of the muscles corresponding to the area involved. Henry K., aged twenty-six ; German, a laborer ; entered Dec. 22, 1882. He had been struck by a brick falling from a building, and had a compound depressed fracture of the skull slightly to the left of the median line and just posterior to the coronal suture. The area of the fracture was small, and it was slightly depressed, the inner table, however, more than the outer. He was immediately trephined, Dr. McBurney, visiting surgeon, performing the operation, removing the depressed portion. There was no wound of the dura mater. The patient did well until the
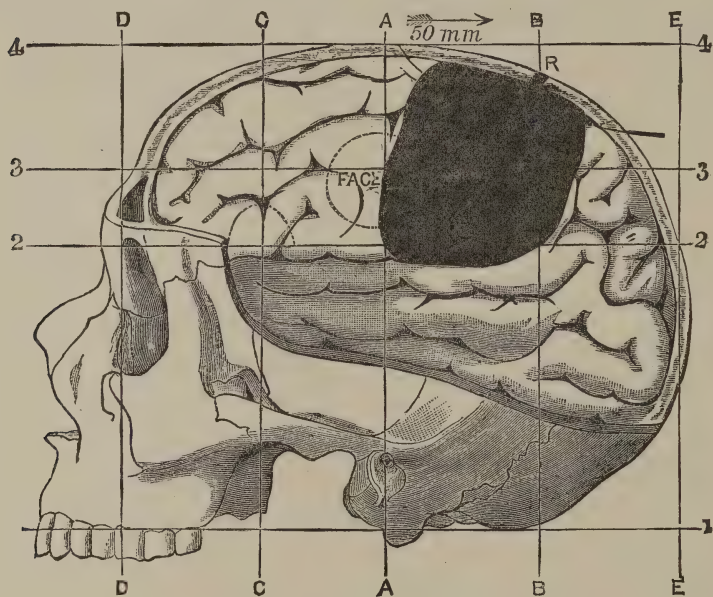
FIG. 3.

sixth day, when symptoms of meningitis developed. There was a steady rise of temperature with occasional vomiting. There were first occasional twitchings of the toes of the right side ; later the leg. Then tonic contraction of the fingers of the right side set in ; this condition later giving place to complete paresis of all parts previously convulsed. His condition grew worse ; high temperature and loss of functional power supervened, the patient dying in coma.

Autopsy.—Eight hours after death showed acute suppurative meningitis of the convexity on the left side. A thick layer of pus covered the left convexity and was especially deep along the

fissure of Rolando. It was quite localized and deeper at this point than at its point of origin—the seat of the trephining. The pus formation involved that part of the longitudinal fissure adjacent to the paracentral lobule, but the remainder of the convexity was normal, and the interior of the brain healthy. The area of the convexity involved in the pus effusion is well shown in the appended diagram (see fig. 3). This was the only case of trephining among
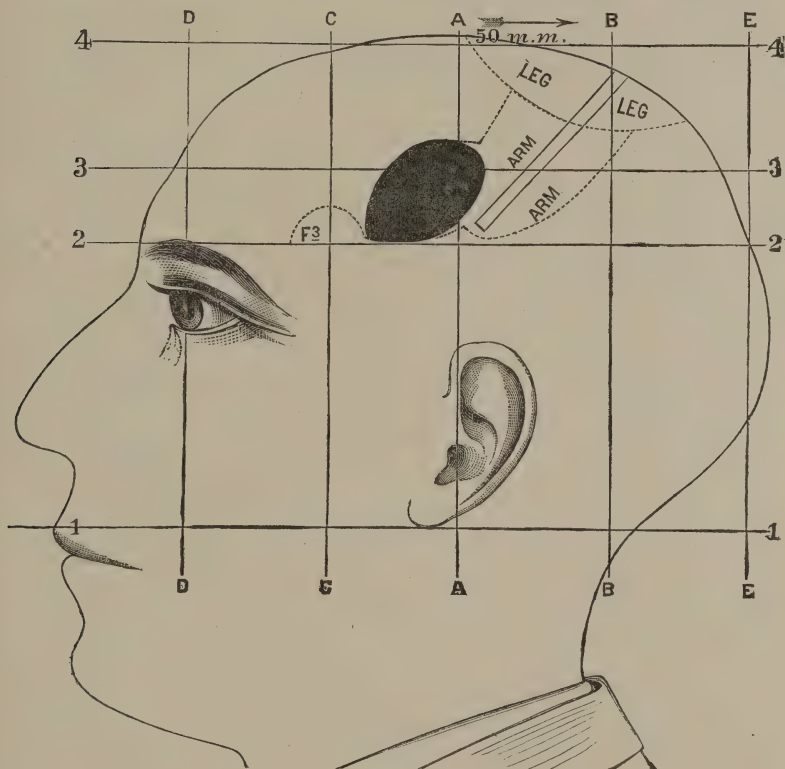
FIG. 4.

a large number that terminated in this manner, and the exact cause is unknown.

CASE 4.—M. N., aged seventeen ; entered March 3, 1883. A short time before admission he fell from the seat of a mail-wagon, striking his head on a car-track. There was no wound of the scalp, but there could be easily felt a marked depression on the right side above and anterior to the ear. The patient walked into the ward, and at the time he was admitted presented no symp-

toms. He soon began to have twitchings of the facial muscles on the left side. This continued for half an hour when left facial paralysis supervened. It was especially marked in the lower half of the face. There was no perceptible deviation of the tongue. The pupils were even and responsive to light. There was no paralysis of the limbs, neither was sensation impaired. Acting upon the instruction of the visiting surgeon, Dr. Robert F. Weir, the house surgeon, Dr. Syms, proceeded to operate. The fracture was quite extensive, and the depressed portion was as large as a silver dollar. It was located at a point above and anterior to the ear, and on a plane slightly above the fronto-lambdoid line. Its location is well indicated in fig. 4. The depressed portion was removed and the edges of bone trimmed with the *rongeur* forceps. The dura was lacerated at one point, from which there was some hemorrhage. The patient was in considerable shock after the operation, but rallied during the night. The next day he was in fair condition, and the facial paresis began to disappear. This together with his general recovery was rapid, and on March 18th, he was allowed to get up. His subsequent recovery was complete.

These four cases can be placed, I think, in the list of contributions to cerebral topography. It is to be regretted that more accurate cranial measurements were not taken, but in these, and it is the case in the majority of cases of trephining for one cause or another, there were local indications pointing to the area to be trephined. Measurements should, however, always be made for confirmatory if not for diagnostic reasons. As far as I have been able to ascertain from published cases, there are only four on record where the operation was performed based solely upon cranial measurements. These were the cases of Broca and Lucas-Championierre, Dr. Weir's case, and one of Dr. Seguin's operated upon by Dr. Sands at Roosevelt Hospital ; and the accuracy of these measurements was verified in a brilliant manner.

As regards parallel cases to the first case in this article, that of traumatic epilepsy, we have a moderately large

number on record. The most complete collection is that of Mr. Walsham, to be found in St. Bartholomew's Hospital reports, vol. 19. Here are collected detailed histories of eighty-two cases, of which forty-eight were completely cured, thirteen were improved, four not improved, and seventeen died.

As regards the cause, the nature, and the situation of the head lesion to which the epilepsy was ascribed, in seventy-two out of the eighty-two cases of epilepsy, a distinct lesion standing in the relation of cause and effect was noted, the violence varying from a severe injury to a slight blow. The *nature* of the injury varied from a simple contusion to a compound depressed fracture. The *seat* of the head injury varied, but in nearly two thirds of the cases it was situated over either one or the other parietal bone. This however, I think applies to all head injuries. As regards the time of onset, duration and character of the symptoms, the convulsions appeared in a space of time varying from a few hours to several years, and the duration of the attack varied approximately.

Convulsions were generally the chief symptom. Pain was present in one half the cases. In some it was persistent, in others only excited by pressure on the scar or seat of injury.

In twenty, paralysis existed, in addition to the epilepsy, varying from partial loss of power to complete hemiplegia. In one there was paralysis of the muscles of the face. In three there was aphasia.

In regard to the existence of local indications for the seat of trephining in the eighty-two cases, there were in seventy-nine local evidences distinguishing the injured area, and, I may here repeat, it is these local lesions in the great majority of cases that have guided the operator, to the exclusion of cranial measurements. In forty-four of

the eighty-two cases, there was a deep cicatrix or a depressed spot painful on pressure, sensitive, or the seat of constant pain. Pressure in two cases caused vertigo, in one case convulsions, in one case rigidity of the arm. In one the temperature was 3° F. higher than over any other part of the head. In a few cases there were sinuses leading down to dead bone.

As regards the *condition* of the parts discovered at the operation, in twenty-nine cases there was depressed bone or depression of the inner table pressing on or projecting into the brain. The dura mater in three cases was adherent, in seven it was penetrated by spiculæ of bone. In one there was a hard scar of its substance,—the result of a former wound. In twenty-one the bone had undergone alteration, —was thickened, carious, thin, necrosed, or porous. In three there was exostosis.

As regards the result of the operation and the probable cause of death in the fatal cases, in the eighty-two, sixty-five recovered and seventeen died. Of the sixty-five that recovered, forty-seven were completely cured, thirteen were improved, four were not improved. In nineteen of the forty-seven cures no fit recurred after the operation; in eighteen there were one, two, or more convulsions in the first few days; in eleven it was not stated, but the cure was said to be complete.

Of the seventeen fatal cases, in two the head was found healthy on autopsy. The most prominent cause of death was meningitis, of which there were seven cases. The mortality was seventeen in eighty-two, or twenty per cent. The situation of the lesion was not accurately stated, but from the appended tables the following deductions are made:

1. Where mania, idiocy, or mental deterioration occurred the lesion may be generally said to have existed in the anterior half of the cranium.

2. Where paralysis existed, the parietal bone with the exception of two cases was the part injured.

In four cases the paralysis affected the arm more than the leg, and it is interesting to note that in the only one of these in which the exact position of the lesion was given it was over the convolutions around the anterior and lower half of the fissure of Rolando. In two of the three cases in which aphasia was a symptom the surface over Broca's area was involved. In all cases with but very few exceptions there were local indications pointing to the area to be trephined.

The experience of Dr. Hammond compares very favorably with the cases collected by Mr. Walsham, presenting in addition a much smaller percentage of mortality, as in thirty cases only one died. In these thirty cases there were nine complete cures, although in two of these there was no injury of the skull to be discovered. Of the remaining twenty-one cases there was no cranial injury in three, and in eighteen cases, although there was an injury, the operation proved unsuccessful. As was stated above, one case died.

The second case in my paper—that of compound depressed fracture, operated upon on the day of injury,—illustrates the application of antiseptic surgery to cases of this description. Primary union of the scalp with complete healing under two dressings stands in strong contrast to the slow healing and homely cicatrix existing under the old method of treatment, where the wound was packed with lint soaked in some styptic or healing solution, and allowed to granulate.

In a trephining conducted according to the present antiseptic practice, the head is shaved, and the neighborhood of the wound thoroughly cleansed. Antiseptic solutions are used for the instruments, hands, etc., and continuous or

frequent irrigation, generally with bichloride of mercury solution, employed.

All vessels are ligated with cat-gut, and the wound sutured with the same, first trimming its borders of all ragged integument and hair follicles. Drainage tubes may be employed or not as the operator decides. An antiseptic dressing of gauze and cotton or peat is applied, and not disturbed for a week or more, when the wound should be found healed. Primary union is almost always obtained as in ordinary scalp wounds, and is much preferable to the elevated and inverted cicatrix of the granulating wound, and in two cases I have seen necrosis of the bone result in the slow granulating wound.

In a case of my own not here cited, one of compound depressed fracture without symptoms, treated strictly anti-septically, primary union of the wound under *one* dressing-place was obtained, drainage tubes being here dispensed with.

The fourth case in my paper was a simple fracture with depression, but with symptoms indicating cortical pressure. Trephining was justifiable, and early operation in these cases I think is preferable to delaying until severe symptoms arise. This, however, is a contested point, and more proper for discussion by those whose experience has extended over a longer time than has my own.

THE NEGATIVE ARTERIAL PULSE AND NEGA-
TIVE ARTERIAL TRACE.

By A. T. KEYT, M.D.

THERE is a normal negative arterial pulse. The fol-
lowing are established physiological facts :

1. The arteries expand during systole, and return to
their original calibre during diastole.

2. The arteries elongate during systole, and return to
their original length during diastole.

3. The movements of expansion and return, and of
elongation and return, are synchronous, and due alike to
the advance and recession of the wave of blood from the
heart.

4. The positive arterial pulse is co-incident with the
movements described and produced likewise by the wave of
blood from the heart.

5. The arteries are thrown into curvatures in systole
and redressed in diastole. This rhythmic flexion or loco-
motion of the arteries is a necessary consequence of their
elongation between fixed points; besides, it is a matter of
direct observation.

With these facts, which no one will question, we are pre-
pared to make the following deduction :

As the curvature is formed in systole the convex aspect
pushes out the contiguous tissues, and the concave aspect
simultaneously draws in the contiguous tissues. As the

curvature is redressed in diastole, the tissues that were pro-
truded fall back to their original place, and the tissues that
were retracted rise up to their original place.

Then the integuments over an artery in which there is
distinct locomotion will present two lateral spots, of which,
in systole, one rises whilst the other falls, and in diastole
one falls whilst the other rises.

The systolic rise and diastolic fall answer to the positive
pulse of arterial locomotion, while the systolic fall and
diastolic rise answer to the negative pulse of arterial
locomotion.

It is apparent that arterial expansion, adding itself to
arterial locomotion, while increasing the positive, tends to
lessen the negative, pulse.

The aspiration we speak of is as real as arterial lengthen-
ing and lateral locomotion. There are no physical con-
ditions to defeat it. The movement of the artery to one
side tends to create a vacuum, which is immediately filled
by sinking in of the skin covering the part which the artery
vacates. If lateral locomotion of the arteries is accepted as
a physiological fact, there is no escape from the conclusion
of the existence of arterial aspiration and a negative arterial
pulse.

Demonstrations.—Besides this *a priori* evidence in favor
of a negative arterial pulse, it is susceptible of direct
demonstration.

If in persons with visibly pulsating arteries we watch at-
tentively the movements, we will see an unequivocal simul-
taneous rise of the skin at one spot and fall at another. In
some persons the retraction is almost as marked as the pro-
trusion. The phenomenon I have often seen and pointed
out to others, who immediately confirmed its reality.

It may be further beautifully demonstrated by placing
over the spot of systolic recession a very delicate Hérisson

sphygmometer, whose rim and membrane are of small diameter (the instrument carefully placed so as to exclude the agency of impact against the rim). In this experiment the liquid column is seen to suddenly fall and gradually rise, exactly inversely with the sudden rise and gradual fall observed when the instrument is over the spot of systolic protrusion.

Another way of demonstrating the negative arterial pulse is by graphic traces. The experiment, however, is not re- markably easy, and requires for its successful performance careful manipulation with a very sensitive sphygmograph and special explorer. The difficulty lies in the fact that, when an ordinary explorer is placed to obtain the effect of aspiration, the rim, if it be a membrane explorer, or, the supporting bar, if it be a spring explorer, rests directly upon the artery and receives the effect of impaction of the pulse- wave. The effect of this impaction, as pointed out by Dr. S. E. Post, is to cause the membrane or pad of the explorer to fall and so carry the line downward in systole. In this case the line falls both from arterial displacement and ele- vation of the frame by the lifting force of the pulse-wave ; and the trace so obtained divides its mechanism of produc- tion between the two factors. The position most favorable for aspiration is the one most favorable for impaction, and it seems impracticable to place the ordinary explorer so as to receive the former effect without the latter. The com- bined result is an inverted trace more ample and pro- nounced than could be produced by displacement alone.

However, with the delicate sphygmometer, before re- ferred to, attached to my sphygmograph for an explorer, traces may be obtained of the pulse of aspiration entirely free from the element of impaction. The trace is very small, but so is the trace of the positive pulse taken with the same explorer. Figure A is an example of such trace

obtained from a lady whose radial pulsated visibly and with distinct systolic aspiration.   Doubtless if the negative trace of displacement in its purity could be obtained with the
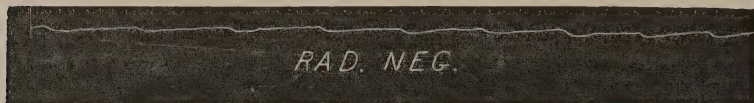
FIG. A.

usual larger explorer the result .would be more pronounced.

With these evidences in support of the negative arterial pulse we feel justified in submitting it as a true physiological phenomenon.

THE NEGATIVE ARTERIAL TRACE.

In a late paper [2] I gave examples of inverted traces obtained when the explorer was placed by the side of the arteries instead of directly over them, and gave the demonstration of the arterial origin of these negative traces—an idea which it seems had not been previously entertained. I showed that the negative trace so obtained presented the same general form as the positive arterial; that it fell as the positive rose, and rose as the positive fell ; that it presented the same number and position of secondary waves, the same aortic notch, as the positive ; and that it was ex-
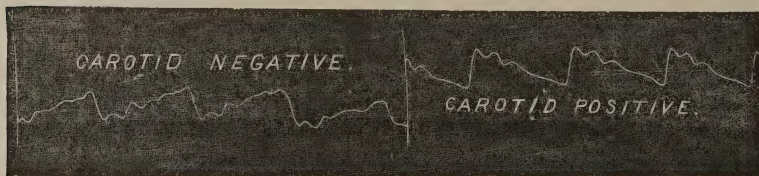
FIG. I.

actly synchronous with the positive in primary start, and each individual phase throughout. These characteristics and correspondences were considered to stamp this negative trace as produced alone by the arterial movements. (See figures.)

[2] *Boston Medical and Surgical Journal*, October 11, 1883, p. 310.

In seeking an explanation of the phenomenon, I remarked that its mode of production had been to me a puzzling problem, but that I then believed its solution was found in the locomotion of the arteries. I applied the mechanism of production of the negative arterial pulse in explanation of the negative arterial trace. This theory is still correct as a factor of the mechanism, but I now admit that it is not
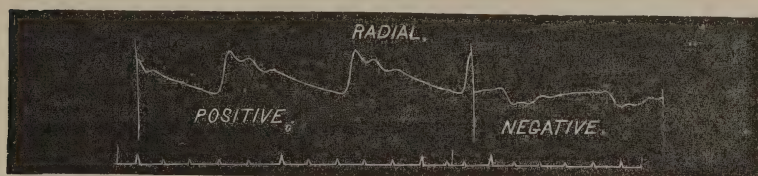
FIG. 2.
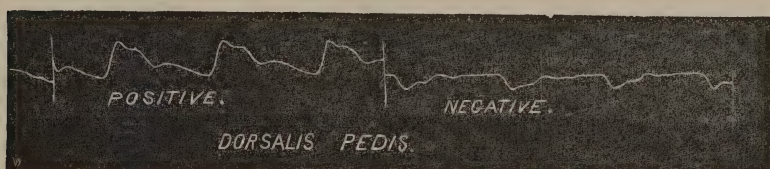
FIG. 3.
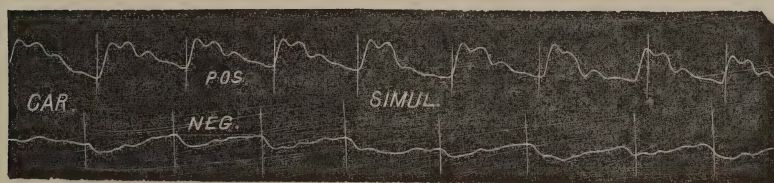
FIG. 4.

sufficient to wholly explain the trace as ordinarily obtained ; there is another important factor.

Dr. S. E. Post, after conversion from the theory of venous to that of arterial origin of this trace, has divined and pointed out the agency in its production of " direct impact against the frame instead of against the diaphragm of the explorer." [3]   There can be no

[3] See ARCHIVES OF MEDICINE, February, 1884.

question that such "frame-impact" is capable of causing the diaphragm to descend in systole and ascend in diastole, and of reflecting in inverted relation all the details of the arterial pulse. I gladly accept this agency as the stronger factor in the ordinary production of the negative arterial trace.

THE THERMIC PHENOMENA IN CONTRACTION OF MAMMALIAN MUSCLES.

(Second communication from the Leipzig Physiological Institute [1]).

By ROBT. MEADE SMITH, M.D.,

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NEARLY all the information which we possess con- cerning the thermic and electric phenomena of muscular contraction has been acquired through the study of the excised frog's muscle ; and if we could disregard the fact that every stimulation of a muscle diminishes its store of irritable matters concerned in the production of contrac- tion, and at the same time causes an accumulation of the waste products of contraction, and to that extent reduces the potential energy of the muscle, we might hope that the study of the isolated muscles of cold-blooded animals would ultimately reveal all the conditions of their functional ac- tivity.

But we know that with every repetition of irritation there is a continuous decrease in all the muscular activities, point- ing not only to a marked diminution in the supply of con- tractile material, but, as Heidenhain has shown, to still more complex alterations, as evidenced by the unequal de- crease in the power of doing work and of heat production.

If we compare the excised muscle with the living muscle still in the circulation, we find that the latter may be sub-

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[1] See Du Bois Reymond's *Archiv für Anatomie und Physiologie*, Physiolo- gische Abtheilung, 1881, p. 105.

jected to a much more prolonged stimulation without losing
its capability of doing work, and that in the interval of rest
following the exhaustion consequent upon prolonged con-
traction, its energy is restored much more quickly and com-
pletely than when the changes produced by the circulation
are impossible.

From the fact that the bloodless exhausted muscle is able
by rest to regain a portion of its contractile force, we must
conclude that in the interior of the muscle there is a store
of material which is capable, even without the influence of
the blood, of being converted into irritable contractile mat-
ter. And although these matters must originally be derived
from the blood, a continuous contact of muscle and blood is
not essential to their final development ; though the com-
parison of the bloodless and excised muscle shows that
this accumulation and development is facilitated by the
blood-stream. Consequently the blood gives to the muscle
a greater power of prolonged contraction, and, therefore, in-
creases its power of doing work; while an examination
of the gases of the arterial and venous blood of con-
tracting muscles gives us some idea of the nature of the
chemical changes occurring in the muscle, and the share
taken by the blood in such processes.

The question arises whether the influence which we have
seen that the circulation exerts on the work accomplished
by the muscle also applies to the heat production in mus-
cular contraction.

In my first communication on this subject I reported a
number of experiments which seemed to prove that the
bloodless muscle not only did not become as warm in con-
traction as the muscle still in the circulation, but also that
the actual heat production was much less. The method of
closing the circulation in the muscles then employed was,
however, a tedious one, and necessitated the removal of the

coverings from the limbs of the animal ; thus, by permitting cooling of the skin, interfering with one of the requirements of the experiment. . A new method of closing the circulation, suggested to me by Prof. Ludwig, ensures much purer results, and permits of greater variation in the conditions of the experiments. This method rests on the fact that when hydrostatic pressure is brought to bear on the interior of a rubber tube, its walls do not dilate uniformly, but a ball-like expansion forms at a single point; consequently, if a narrow thin-walled rubber tube, one end being closed, is inserted through the left axillary artery into the dorsal aorta, the lumen of the latter may be entirely obliterated by forcing water into the rubber tube.

To accomplish this a piece about 100 mm. long was cut out of a flexible elastic catheter, with an external diameter of about 2 mm., on to one end of which a piece of thin rubber tube about 30 or 40 mm. long was tightly fastened with thread ; a hollow conical metal point was fastened in the free end of the rubber tube. Into the other end of the catheter was fastened a metal tube and stop-cock which fitted the nozzle of a small syringe. In order to facilitate the insertion of the flexible catheter, a wire stylet was passed through its interior into the metal point in the rubber tube. Half per cent. salt solution was employed to dilate the tube, the amount which could be injected without bursting the tube being always determined by measurement before the commencement of the experiment.

In order to permit of closure of the circulation through the descending aorta, the left axillary artery is exposed in a dog, when it crosses the first rib, all branches tied, and the main trunk surrounded by double ligatures. The vessel is then opened between the two ligatures, and the catheter and tube inserted, and, after loosening the lower ligature, pushed down into the descending aorta. In order to de-

termine whether the bulb lies in the proper position, and whether its dilatation completely occludes the descending aorta, the femoral artery is connected with a mercurial manometer ; if the bulb is then dilated by the injection of the salt solution so as to occlude the aorta, the pulse instantly ceases in the vessels of the lower extremity, and the blood pressure, as measured by the manometer, sinks within a few seconds nearly to zero, while the circulation continues in the head and right upper extremity. If the contents of the tube which connects the femoral artery with the manometer are pressed into the former, the column of mercury in the manometer sinks below zero and remains constant as long as the occlusion of the aorta continues perfect, showing an absolute cessation of all circulation in the vessels of the lower extremities. If the stop-cock on the catheter is then opened, the contents of the rubber tube are expelled, and instantly the column of mercury in the manometer commences to pulsate and to show a rapidly increasing blood-pressure in the femoral artery. As might be expected, however, the original blood-pressure in these vessels is not immediately regained, but only after the relaxed empty vessels regain their normal tone ; this usually occurs within a few minutes, as shown by the following examples :

I.—Pressure in the femoral artery before closure of the aorta was 134 mm. Hg. The aorta was closed for five minutes, during which time the pressure was zero. Thirty seconds after reopening the aorta the pressure was 65 mm., and four minutes later was 120 mm.

II.—Pressure in the femoral artery before closure of the aorta was 100 mm. Hg. The aorta was closed for fifteen minutes, during which time the pressure remained at zero, and one minute after reopening the aorta the pressure stood at 100 mm.

Occasionally, when the closure is very prolonged, or often repeated, the pressure in the manometer will be noticed to

rise gradually and slightly above the zero point, probably from the dilatation of the aorta at the point of closure, or from the expansion of the rubber tubing losing its globular form and becoming more elongated. All that is then necessary is to inject more fluid into the rubber tube when the occlusion will again become complete, and the pressure again fall to zero.

The rubber catheter was always inserted at the commencement of an experiment and allowed to remain throughout its entire course, as it was found that its presence in the undilated condition appeared to be without influence on the circulation, and coagulation was never produced even when the experiment lasted several hours.

Since, as proved by post-mortem examination, the aorta in my experiments, which were made on dogs weighing from twenty to twenty-five kilo., was always occluded about midway between the origin of the axillary artery and the diaphragm, when the closure was prolonged more than a few minutes it invariably produced symptoms of dyspnœa from the interference with the circulation of the diaphragm. This was always counteracted by artificial respiration.

In other respects the experiments were conducted in about the same manner as already described in my first paper (*loc. cit*, pp. 106, 107).

Throughout the entire course of the experiment, the animals were kept profoundly narcotized by intravenous injections of chloral solution. The thermometers employed were carefully compared with one another, were graduated into $\frac{1}{10}°$ C., and by means of a magnifying-glass $\frac{1}{100}°$ C. could readily be estimated with accuracy. One thermometer was inserted through the right carotid artery into the arch of the aorta to measure the temperature of the arterial blood ; another was passed through a small incision in the skin deep into the furrow between the *vastus externus* and

rectus cruris of the right thigh, to measure the temperature
of the muscles. The crural nerve was divided at Poupart's
ligament and the peripheral end placed on shielded elec-
trodes connected with an induction apparatus. The con-
dyles of the right femur were clamped to the table and
a weight of five hundred grm. so fastened on to the
freely movable lower extremity that contraction of the
extensor muscles of the leg raised the weight and re-
corded the height of the lift on the moving paper of the
kymographion. The entire right inferior extremity was
wrapped with the greatest care with several layers of cot-
ton batting and allowed to remain undisturbed throughout
the entire course of the experiment; by this means the
temperature of the skin was kept within 0.7° or 0.8° C. of
that of the resting muscles. Most of the experiments were
made in May, June, and July, 1883.

The first point to be determined was, whether the blood,
under the conditions under which the experiments were
conducted, was the only channel by which heat was removed
from the mass of the muscles. That this is the case is
proved by the fact that the temperature of resting muscles,
as measured by a thermometer imbedded in their mass, re-
mains unchanged by arrest of their circulation through
closure of the aorta; but when the blood current is restored
there is a rapid change in the muscular temperature, the
character of the change depending upon the relative tem-
peratures of the muscle and arterial blood. Therefore, be-
fore considering this point further, the changes in tempera-
ture of the arterial blood produced by arrest of circulation
through the aorta must be first considered.

Examination of the thermometer whose bulb lay in the
arch of the aorta showed that an invariable result of the
closure of the descending aorta was a decrease of tempera-
ture in the volume of blood still remaining in circulation.

The following examples taken from four experiments made on different animals furnish illustrations of the degree and course of this reduction in temperature.

I.	Time after Closure of the Aorta (Minutes).						
	0	1	2	3	4	5	6
Temperature of blood	36.15	36.05	35.95	35.90	35.85	35.80	35.76° C.
Loss of temperature in each minute	. . .	0.10	0.10	0.05	0.05	0.05	0.04° C.

Total loss of temperature in closure of aorta of six minutes' duration = 0.39° C.

II.							
	0	1	3	5	7	9	11
Temperature of blood	34.56	34.42	34.26	34.22	34.20	34.20	34.20° C.
Loss of temperature in every two minutes	. .	0.14	0.8	0.2	0.1	0.0	0.0

Total loss of temperature in closure of aorta of eleven minutes' duration = 0.36° C. in the first seven minutes ; without change in the four following minutes.

III.					
	0	1	2	5	10
Temperature of blood	38.36	38.15	38.13	38.06	38.06° C.
Loss of temperature in each interval	. . .	0.21	0.02	0.07	0.00

Total loss of temperature in closure of aorta of ten minutes' duration = 0.30° C. in the first five minutes ; without change in the five following minutes.

IV.							
	0	1	3	7	8	10	15
Temperature of blood	39.50	39.30	39.32	39.27	39.25	39.20	39.20° C.
Loss of temperature in each interval	. .	0.20	+0.02	0.05	0.02	0.05	0.00

Total loss of temperature in a closure of the aorta of fifteen minutes' duration = 0.30° C. in the first ten minutes ; without change in the five following minutes.

The above experiments show that the backing up of the blood in the upper half of the body changes the normal relation between heat production and heat dissipation with a consequent reduction in temperature of the circulating blood, at first rapid but soon reaching its maximum. Even when a new source of heat is developed in the lower extremity the temperature of

the arterial blood in the upper portions of the body follows precisely the same changes as detailed above, provided that all circulation between the upper and inferior portions of the body is prevented.   If, however, the occlusion of the aorta be not complete and a certain amount of blood circulate through the lower extremities, a development of heat in those portions of the body will cause an elevation of arterial temperature as measured in the arch of the aorta.   Thus in the last of the above illustrations, the slight increase in temperature in the third minute after occlusion of the aorta was coincident with a rise of the column of mercury in the manometer in connection with the femoral artery; when, however, a new quantity of fluid was injected into the rubber tube and the arterial pressure in the lower extremities again reduced to zero, it was found that the temporary rise was again replaced by a fall in the temperature.   Indeed it was found that the temperature of the arterial blood was a very good index of the degree of closure of the aorta, an incomplete occlusion being evidenced by a rise of arterial temperature almost as soon as by the rise in the mercurial manometer.

As might be expected, when, after closure for a number of minutes, the aorta is again rendered patulous, the fall of temperature in the arterial blood is replaced by a rise ; the remarkable fact was however noticed that the temperature of the arterial blood after opening of the aorta rises above its original height, even though the animal has remained motionless and in deep narcosis during the entire period of aortic obstruction.   The following examples obtained from experiments on three different animals illustrate this point :

		Before closure of aorta.	At end of closure of aorta.	After reopening the aorta.
1.	Arterial Temperature .	39.18° C.	39.09° C.	39.27° C.
2.	" " .	37.97° "	37.70° "	38.10° "
3.	" " .	39.23° "	39.05° "	39.30° "

Consequently, if the blood which has been reduced in temperature by closure of the aorta becomes heated above its original temperature by circulating through the parts which had been deprived of blood, it follows that heat must have been accumulated in those organs even in the absence of the circulation. Although not lying within the immediate scope of the present investigation, it seemed to me worth an attempt to determine whether this heat production in the bloodless organs was at all analogous to the increase of temperature which often occurs after death.

A dog, deeply narcotized with chloral, was well covered with cotton wadding, and accurately compared themometers were inserted under the skin of the thigh, between the extensor muscles of the leg, against the femur, in the vagina, and through the right carotid artery into the arch of the aorta.

	Skin.	Bone.	Muscle.	Vagina.	Arterial Blood.
Before closure of the aorta	37.67	38.30	38.38	38.25	37.97° C.
Three minutes after complete closure of the aorta . .	37.61	38.32	38.38	38.32	37.70° C.

After reopening the aorta, the animal was allowed to remain undisturbed for seventeen minutes, and the aorta again closed. The temperature-changes were as follows:

	Skin.	Bone.	Muscle.	Vagina.	Arterial Blood.
Before second closure or the aorta .	37.60	38.30	38.41	38.38	38.13° C.
Nine minutes after closure	37.10	38.37	38.48	38.50	37.85° C.
Temperature-changes	— 0.50	+ 0.07	+ 0.07	+ 0.12	— 0.28° C.

The temperature then began to fall in all the situations where a rise had been already noted, and in six minutes— that is, fifteen minutes after the closure of the aorta—the temperature stood as follows:

Skin.	Bone.	Muscle.	Vagina.	Arterial Blood.
36.85	38.35	38.42	38.45	37.80° C.

The aorta was then opened, and nine minutes later the following temperatures were noted :

Skin.	Bone.	Muscle.	Vagina.	Arterial Blood.
37.28	38.20	38.28	38.37	37.90° C.

The experiment therefore proves the truth of the supposition under which it was undertaken, viz.: that under the existing circumstances, when the circulation is arrested, more heat is developed in the bloodless parts than can be dissipated. When, however, the circulation is restored, this excess of heat production over heat dissipation is removed by the blood.

This experiment, moreover, throws special light on the view which attributes an increase or decrease in temperature of a given region to a corresponding change in its blood supply, as it is evident that an increased blood supply must produce varying results, according to the relations between its own temperature and that of the given region, and how unwarrantable it is to apply without qualification to the muscle results which are applicable to the skin. For with a larger supply of blood the cool skin, even though exposed to excessive and rapid loss of heat, will become warmer, while on the other hand the warmer muscles will become cooler. Consequently the conception must be erroneous, which is generally held as to the temperature changes in muscle from alteration in their blood supply after section of their nerves.

In order to permit of generalization of the results of the above experiment for the further prosecution of this subject, it was necessary to determine whether the resting muscle and muscle recovered from the exhaustion of prolonged contraction were capable, during interruption of their blood supply, of developing sufficient heat to cause an increase in their temperature. It is evident that if the tissue-change in bloodless muscles, which at best must be insig-

nificant, is able to develop enough heat to be measured by the thermometer, it can be assumed with certainty that the locality in which the increase of temperature is measured is practically removed from all sources of cooling.

The following results, obtained from experiments on six different animals, are selected from a large number of similar observations.

TEMPERATURE OF THE RESTING MUSCLE DURING CLOSURE OF THE AORTA.		TEMPERATURE OF THE ARTERIAL BLOOD DURING CLOSURE OF THE AORTA.		
At Commencement.	At End.	At Commencement.	At End.	Duration of Closure of Aorta.
I. 34.63° C.	34.64°	34.59°	34.50°	5 min.
34.53	34.59	34.56	34.26	5 "
34.50	34.50	34.32	33.88	5 "
34.31	34.34	34.18	33.79	5 "
II. 39.07	39.11	39.18	39.09	5 min.
39.91	39.94	39.60	39.37	2 "
III. 38.21	38.27	38.06	37.95	5 min.
38.18	38.27	38.10	37.80	5 "
IV. 36.14	36.17	36.15	35.76	5 min.
36.19	36.21	36.10	35.80	5 "
V. 39.88	39.96	39.90	39.78	5 min.
39.81	39.91	39.90	39.85	5 "
40.06	40.08	40.02	39.93	5 "
VI. 39.60	39.64	39.40	39.13	15 min.
39.80	39.87	39.20	39.20	9 "
39.58	39.62	39.27	39.05	4 "

The great uniformity of the above results demonstrates that the resting muscles of warm-blooded animals develop heat independently of the blood current, and that in the method employed the heat developed by the bloodless muscle is but slowly given off.

In the contracting muscle, the conditions are different

from those of muscles at rest; the former becomes very much warmer than its surrounding media, while it is probable that it is exposed to a much more considerable loss of heat, the extent and rapidity of the latter being capable of measurement by first stimulating either the normal or bloodless muscle as long as the temperature continues to rise, and then noting the changes in the temperature of the muscle during interruption of the blood supply. The increase in temperature will evidently vary with the mass of the muscle and the difference between its temperature and that of the surrounding media. In my experiments, dogs of about the same size were always selected; therefore, the first source of error in generalizing the results was to a certain extent eliminated, while the differences in temperature between the muscles at rest and in contraction, in the different instances, varied in the widest extremes, thus filling the second condition of the experiment.

In the following series, the muscles were first tetanized with an induction current and the circulation through the aorta arrested at the moment the irritation ceased.

TEMPERATURE OF THE MUSCLE.			
At Commencement of Aortic Closure.	At End of Aortic Closure.	Duration of Aortic Closure.	Increase of Muscle Temp. at End of Irritation.
Exp.			
3.— 39.93	39.93	2 minutes.	0.47° C.
38.70	38.65	3 "	0.49° "
38.90	38.88	5 "	0.48° "
38.81	38.80	6 "	0.48° "
8.— 39.70	39.67	5 "	0.44° "
39.96	39.93	6 "	0.49° "
6.— 36.62	36.55	6 "	0.43° "
7.— 36.77	36.66	4.5 "	0.83° "

While it may be stated from the preceding figures that, with a varying difference in temperature of from 0.49° to 0.43° C. between the bloodless muscle after contraction and the surrounding media, the muscle only lost 0.01° C. in

temperature in every minute, (or 0.02° C. where the differ-
ence was 0.83° C.), this statement can only be held to give
the results in a general way; since naturally with the reduc-
tion in temperature of the muscle the difference between
its temperature and of its surroundings will also decrease,
and consequently the loss of heat by the muscle must be
hindered—if conduction to the surrounding media is the
sole source of loss of heat. An examination of my figures
has, however, shown me that the condition is a more com-
plex one, for often when the difference in temperature is
most marked, as at the end of contraction, the reduction of
temperature is at first slow and then subsequently rapid;
pointing to the fact that after contraction has ceased there
may still be a subsequent development of heat in the blood-
less muscle which will tend to balance the loss of heat by
external conduction. Nevertheless, from the general con-
formity of results, it is warrantable to conclude that in
each minute after contraction the bloodless muscle loses
about two per cent. of the difference between its tempera-
ture and that of the surrounding media.

The share of the circulation in abstracting heat from con-
tracting muscles was proven in my first communication by
the comparison of the temperatures of contracting muscles
and that of their arterial and venous blood, in which it was
found that the excess of temperature of the venous blood
over that of the arterial increased, within certain limits, pro-
portionally with the increase in temperature of the muscle.
This explanation is confirmed by another mode of expla-
nation, which proves in the clearest manner the important
rôle played by the circulation in the reduction of muscular
temperature.

When the muscle is thrown into tetanic contraction and
the circulation arrested just before the cessation of irrita-
tion, as already pointed out, the temperature of the resting

bloodless muscle after contraction remains almost constant. If the aorta is then opened the muscular temperature instantly commences to fall, the rapidity of decrease being proportionate to the difference between the temperatures of the arterial blood and that of the muscle; therefore the fall which at first is very rapid becomes slower and slower as the two temperatures approach one another. Thus, for example, in cases where the muscle was from 0.7° to 0.8° C., warmer than the arterial blood, in the first minute after restoring the circulation the temperature of the muscle was reduced from 0.2° to 0.4° C.

Under exceptional circumstances when the irritation and closure of the aorta are simultaneously stopped, instead of the ordinary rapid reduction of temperature, if the muscle is only slightly warmer than the blood, say 0.2° C., the muscular temperature may increase with the restoration of circulation. This increase of temperature, which is always slight, never more than 0.02° C., may continue for a minute or more, and throws considerable light on the conditions under which the muscles develop heat, while the rarity and exceptional conditions of its occurrence prevent it from militating against the previous statements as to the usual cooling action of the blood.

Of the results obtained in the measurement of the heat developed by contracting muscles, the simplest are those acquired in experiments made on bloodless muscles; they will therefore be first considered.

The possible variations of the conditions of the experiment are the following: Tetanus may be produced in a perfectly fresh muscle, or in muscles refreshed by an interval of rest after previous contraction: the first condition when, after the necessary preparations, the circulation is arrested and the muscle at once thrown into tetanus; the second, when the muscle had already been deprived of blood

and stimulated. Or, again, the circulation may be arrested simultaneously with the commencement of irritation, or a few minutes sooner, so as to insure the absence of oxygenated blood in the muscle.

The tetanized muscle can only increase in temperature up to a definite limit which varies for different muscles; and the rapidity with which this limit is attained also varies. In order to obtain a conception of the latter element of heat production, the height of the thermometers must be read off at regular short intervals of time, usually every 30 seconds.

In the following tabular statement of experiments made on six different animals, the upper row of figures of each series gives the temperature of the muscle at intervals of every thirty seconds after the commencement of the tetanus, the first figure to the left giving the muscle temperature immediately before the commencement of the irritation; where an interrogation mark (?) occurs, it means that at that time no thermometer reading was made. In the second row of each series is noted in $\frac{1}{100}$° C. the increase of muscle temperature at each half minute of the tetanus, while in the column headed "number of tetani" is noted the number of times the muscles had been thrown into tetanus before the instance whose temperature-changes are given. The last column gives the duration in minutes of closure of the aorta before the irritation was commenced.

Though not absolutely uniform the above results are sufficiently constant to permit of their statement in general terms. Immediately after the commencement of the tetanus the temperature of the muscles commences at once to rise, so that the increase in temperature acquired in the first half minute either exceeds or is about equal to that of the remaining period of contraction. Since the thermometer does not instantly acquire the temperature of its surrounding

Each reading cell is shown as *temperature / increment*. Blank cells and "?" are as printed.

	Initial	1	2	3	4	5	6	7	8	9	10	11	12	13	Total increase in temperature.	Number of previous tetani.	Duration of previous arrest of circulation.
I.	39.25	39.47 / 0.22	39.61 / 0.14	39.63 / 0.02	39.63 / 0,0	39.63 / 0,0	39.63 / 0. 0								0.38°	0	6 min.
II.	39.26	? / ?	39.65 / 0.39	39.72 / 0.07	39.71 / −0.01										0.46°	0	0
III. a.	34.64	? / ?	34.94 / 0.30	35.01 / 0.07	35.06 / 0.05	35.10 / 0.04	35.14 / 0.04	35.20 / 0.06	35.23 / 0.03	35.25 / 0.02	35.26 / 0.01	35.27 / 0.01	35.29 / 0.02	35.30 / 0.01	0.66°	0	6 min.
b.	34.59	34.78 / 0.19	34.82 / 0.04	34.86 / 0.04	34.92 / 0.06	34.94 / 0.02	34.97 / 0.03	35.01 / 0.04	? / ?	35.06 / 0.05	35.11 / 0.05	35.11 / 0			0.52°	1	6 min.
c.	34.50	34.70 / 0.20	34.78 / 0.08	34.80 / 0.02	34.82 / 0.02	34.82 / 0,0	34.85 / 0.03	34.84 / −0.01							0.34°	3	4 min.
d.	34.34	34.51 / 0.17	34.52 / 0.01	34.54 / 0.02	? / ?	34.54 / 0.02	34.55 / 0.01								0.21°	4	5 min.
IV. a.	38.27	38.51 / 0.24	38.57 / 0.06	38.59 / 0.02	38.60 / 0.01	38.63 / 0.03	38.68 / 0.05	38.68 / 0. 0							0.41°	2	6 min.
b.	38.27	38.47 / 0.20	38.57 / 0.10	38.62 / 0.05	38.65 / 0.03	38.64 / −0.01	38.66 / +0.02								0.39°	4	5 min.
V.	36.21	36.41 / 0.20	36.45 / 0.04	36.44 / −0.01											0.24°	2	5 min.
VI.	39.91	40.07 / 0.16	40.14 / 0.07	40.17 / 0.03	40.18 / 0.01	40.18 / 0	40.18 / 0								0.27°	1	5 min.

medium, the almost instantaneous rise in the column of mercury indicates that the commencement of contraction and heat production are simultaneous. After the first half-minute of contraction the increase of muscle temperature becomes more and more gradual and slow, and finally a point is reached in the duration of contraction when the temperature remains constant, when the heat production and heat dissipation by the muscle are equal,—a confirmation of the results obtained in the temperature measurements of resting bloodless muscles. And since in this instance there was not as great a difference in temperature between the muscles and the surrounding media as in the cases where measurements were made as to the loss of heat by resting muscles, and since I have seen by direct experiment that the thermometers employed were sufficiently sensitive to indicate within ten to twenty seconds changes in the temperature of $\frac{1}{100}$° C., it may be assumed that in the above cases where the thermometer ceased to indicate a rise in temperature, if any production of heat occurred at all, it could not have been more than $\frac{1}{100}$° C., within the minute.

The experiments further show in the uniformity of the results obtained in the instances where the the circulation was arrested simultaneously with the commencement of contraction, and in those where five or six minutes elapsed after the closure of the aorta before the muscle was stimulated, that the amount of oxygen remaining in the latter case in the blood within the muscle is without marked influence on the degree and course of the heat production by the muscle.

On the other hand it is evident that the frequency with which the bloodless muscle is tetanized, after equal intervals of repose in which the circulation is present, is of considerable influence on the heat production by the muscle. Thus in the observations recorded in number III. of the above

series: in *a.* the perfectly fresh muscle, after arrest of circulation, was tetanized with a maximal irritation, the blood current then restored, and the muscle allowed to rest for twenty-one minutes; and in *b.* a second time irritated with maximal currents, again allowed to rest for twenty minutes as before; and then in *c.* irritated for a third time, allowed to rest for sixteen minutes; and in *d.* irritated for a fourth time. In the fresh muscle the increase of temperature was 0.66° C., in its second contraction it was 0.52° C., and in its third contraction 0.34° C., while the times required for reaching the maximum temperature were respectively seven, six, and four minutes; thus showing that a repetition of contraction of muscles deprived of their blood supply causes a reduction in the amount and duration of heat development.

Since it has been shown that, as far as measurements with the thermometer go, the muscle in the absence of the circulation gives up but little heat to its surroundings, but accumulates in its interior all the heat developed in the tetanus, if we accept Rosenthal's estimate of the specific heat of muscle, the amount of heat developed may be expressed in heat-units. Thus the specific heat of muscle being 0.825 (that of water being 1), reducing the maximum and minimum temperature of the above table to heat-units, it may be stated that one gramme of tetanized muscle may develop from 0.54 to 0.17 heat-units.

(To be continued.)

NEW BOOKS AND INSTRUMENTS.

Mental Diseases. By T. S. CLOUSTON, Physician Superintendent of the Royal Edinburgh Asylum for the Insane, Lecturer on Mental Diseases in the Edinburgh University, etc. Pp. 631. J. A. Churchill, London.

This work contains over 600 pages, and is well printed. An apology is made in the preface for the appearance of another work on mental diseases when so many have recently been published, but the author believes that its strictly clinical method of treatment is what is needed by the student and practitioner. They were given as lectures at the Edinburgh University.

The work is divided into nineteen lectures. An enumeration of the headings of each lecture will give an idea of the subjects treated and the scope of the work.

Lecture 1. The Clinical Study of Mental Diseases.

2 and 3. States of Mental Depression—Melancholia (Psychalgia).

4. States of Mental Exaltation—Mania (Psychlampsia).

5. States of Alternation, Periodicity, and Relapse in Mental Disease (Folie Circulaire, Psychorhythm, Folie à Double Forme, Circular Insanity, Periodic Insanity, Recurrent Mania, Katatonia).

6. States of Fixed and Limited Delusion—Monomania (Monopsychosis).

7. States of Mental Enfeeblement (Dementia, Amentia, Psychoparesis, Congenital Imbecility, Idiocy).

8. States of Mental Stupor (Psychocoma).

9. States of Defective Mental Inhibition (Impulsive Insanity, Volitional Insanity, Uncontrollable Impulse, Psychokinesia, Hyperkinesia, Inhibitory Insanity, Insanity without Delusion, Exaltation or Enfeeblement, Affective Insanity) ; The Insane Diathesis.

10. General Paralysis, Paralytic Insanity (from gross brain disease).

11. Epileptic Insanity, Traumatic Insanity.

12. Syphilitic Insanity, Alcoholic Insanity.

13. Rheumatic and Choreic Insanities, Gouty and Podagrous Insanity, Phthisical Insanity.

14. Uterine or Amenorrhœal and Ovarian Insanity, Hysterical Insanity, Insanity of Masturbation.

15. Puerperal Insanity, Insanity of Lactation, Insanity of Pregnancy.

16. The Insanities of the Times of Life, Insanity of Puberty, Insanity of Adolescence.

17. Climacteric Insanity, Senile Insanity.

18. The Rarer and less Important Clinical Varieties of Mental Disturbances.

19. Medico-Legal and Medico-Social Duties of Medical Men in Relation to Insanity.

The author has not over-estimated the value of his work when viewed from the clinical standpoint. The descriptions of the diseases and cases are simple and practical, but true; and one sees as he reads that they are given by one perfectly familiar from daily observation with the cases and diseases he is speaking of. There is one feature of the book which commends it highly, and which is not to be found in any other work on mental diseases as far as I know, and it is the hints and descriptions given as to the practical management and care of the cases, very much as if he were in the hospital ward with you. There is nothing systematic about the arrangement of the subjects treated in each lecture ; this may be disappointing to some readers, but when it is considered that the lectures are intended for students before graduation, or for physicians who do not desire to have an extensive knowledge of this subject, the method of Dr. Clouston certainly commends itself, and in this respect as a class-book it is superior to the Psychological Medicine of Bucknill and Tuke. There are a few things in the work with which I cannot entirely agree ; I will point out a few of them.

In the chapter on melancholia the following forms are distinguished : Simple m., hypochondriacal m., delusional m., excited m., resistive (obstinate) m., epileptiform (convulsive) m., organic (coarse brain disease) m., suicidal or homicidal m. This appears to me an unnecessary division of the subject. Why should there be a form called " resistive " ? It is true we meet with cases who show a disposition to resist being dressed, moved, etc., but this does not appear sufficient reason. And why separate the suicidal

into another form? And the epileptiform makes one suspect that this variety is an association of the melancholia of general paralysis with epileptiform attacks. This excessive division may be said to have the advantage of pointing out to the student a class of cases who, as Dr. Clouston says, resist persistently, doggedly, and unreasonably, and often with fierce violence, all that is done for them, and they are the most trying to the patience of the nurses. This, however, could all be pointed out in the clinical desciption, and we much prefer the division into melancholia simplex, attonita, and agitata.

Under the head of hypochondriacal melancholia two cases are described with this peculiar denomination : " Two cases of visceral melancholia (brothers); delusions that their bowels never moved, etc." This is confusing to a student who is trying to comprehend the nomenclature of mental diseases. There is no reason why a person who believes that his intestines are obstructed should be placed under a different class from the woman who says that one half of her throat is stopped up, and insists that every thing must be put on a certain side of her mouth or she cannot swallow it.

Good, plain descriptions are given of the periodic insanities, the monomanias, and degenerate states.

In the chapter on alcoholic insanity he says, after speaking of hallucinations in alcoholics : " I believe that more suicides and combined suicides and homicides result in the country from alcoholism in its early stages than from any other cause whatsoever."

This is undoubtedly the case in other countries, as well as Great Britain, and a large number of suicides which occur in asylums are probably among alcoholics ; they are the most troublesome class to deal with in this respect.

Treatment is discussed at length and clearly when considering the diseases, and in connection with the relation of cases, and not placed in a separate chapter, as is usually done. Under the treatment of melancholia, after advocating tonics and nutritious diet, the question of medication is taken up, and quinine is placed in the first rank. Opium, he says, does no good, but decided harm by interfering with the appetite and digestion and diminishing the weight. Our experience in this respect differs from that of Dr. Clouston, with this difference, however, that he speaks of opium and our experience is with morphia. For years we have treated melancholia simplex and attonita by sulphate morphia with good results, and found that all our patients improved mentally

and rapidly gained weight. In but one case have we had to discontinue its use, by reason of the constant vomiting which was produced. In all the cases we have treated, now quite a large number, we have not found any loss of appetite or interference with digestion ; constipation has been very slight, and only at the beginning of the treatment. The treatment by morphia has been advocated and carried out for a long time by Voisin and other European physicians.

Dr. Clouston advises in these melancholiacs at night bromide potass. and tinct. cannabis indica. This is not in accord with our practice. The entire treatment of the insane, medical and otherwise, is carefully, plainly, and satisfactorily pointed out.

In a short notice like this it is impossible to give quotations or an analysis of such a book ; it must be read, and we can heartily recommend it to the student and busy general practitioner.

[J. C. S.]

ADDENDUM.—Under the able editorship of Dr. Charles F. Folsom of Boston, Messrs. Henry C. Lea's Son & Co. have issued an American edition of this treatise—a well-printed volume of 550 pages. Dr. Folsom's work consists in an abstract of the lunacy laws and regulations in force in the various States. This matter is arranged alphabetically, and occupies about one hundred pages ; it greatly increases the value of Dr. Clouston's book for the American practitioner.—[EDITOR.]

Surgical Diagnosis. By AMBROSE L. RANNEY, A.M., M.D. Third edition. 8vo, pp. 608. New York : William Wood & Co., 1884.

The appearance of this edition, the third in five years, is evidence that the book has been found useful in professional work. The arrangement of the matter for ready reference, its fulness, and the "royal-road," seductive columns of contrasted symptoms have been its attractions. The present has been freed from many of the errors of the earlier editions. It has been enlarged by the addition of chapters on diagnosis and localization of diseases of the brain and spinal cord, which certainly should have afforded good reason for adding another word or two to the title. The book, improved by illustrations (probably drawn when artists were out of town) has many excellent traits to recommend it.

[J. V. D.]

A Year-Book of Therapeutics for 1883. Edited by ROYAL W. AMIDON, M.D. G. P. Putnam's Sons.

For several years Dr. Amidon has been a successful lecturer on therapeutics,[1] and is therefore most appropriately situated for making such a useful review of the subject as this volume contains. Indeed, as one of the most important functions of a medical teacher is that of presenting to his pupils, each year, the actual state of science on the subjects of his department, the publication of an annual review might profitably be exacted of every one holding this responsible position. A very considerable amount of information is packed closely between the covers of Dr. Amidon's Year-Book. The proportion of space devoted to different subjects is, of course, not necessarily in accord with their intrinsic value, but with the degree of attention they have happened to receive from the year's regiment of investigators.

Dr. Rummo's monograph on iodoform, based on experiments performed in Vulpian's laboratory, is given entire. The translation is made by Dr. George B. Phelps. The monograph occupies forty-seven pages, and the total space devoted to the drug is fifty-four pages. Rummo's investigations, made in Vulpian's laboratory, are mainly physiological, and on phenomena which at present chiefly interest the therapeutist in cases of poisoning. Of the latter, by means of surgical dressings, Koenig has observed forty-eight cases, several fatal, with generalized fatty degeneration of the viscera. No amount (locally applied) under ten grammes proved dangerous.

The main therapeutic interest of iodoform at present attaches to its antiseptic properties. Rummo found that a solution of iodate of soda containing 5 : 1000 was necessary to prevent the development, but a solution ten times stronger was needed to kill germs already developed and actively propagating.

The antiseptic properties of the drug are the basis of an increasing number of therapeutic applications, not only in the dressing of wounds, but of scrofulous and tuberculous sores, in inhalation treatment of chronic pulmonary diseases, in fungoid synovitis, in inflammation (Binz.) ; possibly, also, in diabetes.

The next largest abstract in the Year-Book is on anæsthetics, which occupy twenty-one pages. Of these, thirteen are devoted to a discussion of the dangers of bichloride of methylene. Nine fatal cases, recorded in English medical journals, are discussed by Dr. Junker, and seven favorable cases of his own then related.

The third abstract of any length (twelve pages) sums up Sydney Ringer's experiments on the physiological action of chloride

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[1] At the Woman's Medical College of the N. Y. Infirmary.

of barium ; which he finds singularly similar to that of digitalis. No therapeutic use has yet been found for it. More vivid practical interest therefore attaches to corrosive sublimate, to which ten pages are given. Dr. Weir's paper is quoted, apparently at length. The only other long abstract is from a paper by Nancrede, read before the American Surgical Association, on the local processes of inflammation. This paper is interesting, if only because its suggestions compel the reader to go over again the endless topic of inflammation, and to peer once more into the depths of vital processes. Nevertheless it appears to us rather naïve. The author admits that the typical form of bleeding for arresting inflammation would be from the vein leading from the inflamed part. This is easy to do on a frog's tongue. But how may we do it in pneumonia, or peritonitis, or meningitis, or any other clinical inflammation, such as really tries men's souls ? ·

Many other therapeutical topics are noticed, in concise but comprehensive abstracts of a paragraph or a page. Dr. Amidon, in a general review, alludes especially to the new treatments of phthisis which have been based on the bacillus theory of tuberculosis ; to naphthalin as an antiseptic surgical dressing ; to the newer treatments of malarial and typhoid fever, especially by resorcin, chinolin, carbamide, and kairine ; to convallaria, whose utility seems to be overbalanced by its disadvantages ; to the two new hypnotics, paraldehyde and aretal ; and to the extensive experimentation on transfusion.

A complete alphabetical index lends additional value to this excellent compendium.   [M. P. J.]

A Year-Book of Surgery for 1883. Edited by CHARLES H. KNIGHT, M.D. New York : G. P. Putnam's Sons, 1884.

This new enterprise in American medical writing comes in such good shape and so complete, that we give it hearty welcome and bespeak its permanency. The book, though having not quite two hundred pages, goes well over the ground of the year's work in surgery, and is divided into five parts, which include the surgery of the different regions of the body, general surgery, and venereal disease. The editor here presents his well-chosen cases concisely and intelligibly, with the instruction they were intended to convey. And, what is not a little gratifying, he has made frequent choice of cases from American work, which, although not dazzling, are nevertheless progressive and instructive. An introduction condenses into ten pages the more noteworthy lessons of

the year, which are : impunity in operating through antisepsis ; the place of, and the difficulties connected with, resection of the py-lorus ; thyroidectomy, and its teaching relative to myxœdema ; the relative value of supra-pubic lithotomy ; the value of digital exploration of the bladder through the perineum ; the use of wire suture in transverse fractures of the patella ; the relation of syphilis with rickets and locomotor ataxia ; and the futility of ex-cision of the primary syphilitic sore. .   [J. V .D.]

Veterinary Medicine and Surgery in Diseases and Injuries of the Horse.

Compiled from standard and modern authorities and edited by F. O. KIRBY. Illustrated by four colored plates, and one hundred and sixty wood engravings. New York : William Wood & Co., 1883. Pp. 332.

This work forms the twelfth volume of " Wood's Library of Standard Medical Authors for 1883." The compiler states that it " has been prepared with a desire to present in a concise form a practical manual of the diseases and injuries of the horse and their treatment, for the use of practitioners of medicine and other intelligent horse-owners. The professional veterinarian already has at his command several large and excellent treatises. These, however, devote considerable space to the consideration of subjects familiar to the student of medicine and unnecessary to the non-professional reader, physiology and pathology are therefore seldom referred to in the present work."

It will be seen from the above quotation that the book has a twofold object. It is not only intended for the instruction of the medical profession, exclusive of veterinary practitioners, but is to serve also as a popular treatise. Its aim is even a more ambitious one than that attempted in works entitled " The Family Physician," whose authors are satisfied with catching the popular eye without expecting that they will assume the pretensions of text-books for the medical profession also. Perhaps the compiler, who, we infer, is a non-professional writer, from the absence of a title and the fact that the only claim that he makes to practical medical knowledge is the result of sixteen years' experience in the *ownership* and *consequent* care of horses, is right in classing the medical profession along with the "other intelligent horse-owners," and in separating both classes from " the professional veterinarian "; for, as far as the appreciation of the close relationship which exists between the pathology of the lower animals and that per-taining to man are concerned, it cannot be accredited to the

average physician to a very high degree ; nor is this state of affairs likely to be improved by the work before us ; in which, on the author's own admission, physiology and pathology are seldom referred to. In reading this book the horse-man will have the advantage over the doctor of medicine, for the nomenclature is in the vernacular of the men who "talk horse." He will revel in the terms belly-ache, blind-staggers, warbles, sit-fasts and rat, tails, scratches and seedy-toe ; though the physician may occasionally find relief in a synonym which exhibits an attempt to truly represent a pathological condition in the terms of general pathology.

That the compiler has attained the object set forth in his preface quoted above we do not deny. He has produced a work, however, which must of necessity be a superficial one, and which might well be called "Every Man (Physicians Included) his own Horse-Doctor." Such productions are far from elevating in their effect on veterinary medicine. Works on hygiene, and the application in diseased states, of simple measures, which cannot do harm, with indications as to when to call for professional aid, is all that a popular work on either human or veterinary medicine can with safety attempt.

The details of treatment, and posological statements, while they may be of service to the physician, are as dangerous in the hands of the non-medical horse-owner as our works of materia medica would be in the hands of a layman. Why such a volume should have been added to a "Library of Standard Medical Authors" it is difficult to understand, unless it be a sop thrown to the country practitioner, who is often called upon to apply his medical knowledge to the treatment of disease in the lower animals. To such we would say, that they will do better to rely on their general knowledge of the principles of pathology, than to trust to a brief *résumé* of diseases of the horse, as such compilation must be, however well done ; or else consult some comprehensive work on veterinary medicine, such as William's, Robinson's and others. Let the physician, as well as the layman, remember, however, that veterinary medicine is a specialty by itself, and requires as great skill as any other department in the field of medicine. Valuable animals ought to be intrusted to the care of those who are specially skilled in this practice, rather than to the layman with his dangerous smattering, or even the physician, with his general medical knowledge. The profession and the public should foster the methods of education that fit men for veterinary practice, which is just lifting itself up out of the chaos of quackery.

On the other hand, the scientific student of medicine will do well to devote more time to the investigation of the diseases of lower animals, and not hold himself aloof from his veterinary brother, from whose field of work he may obtain a wealth of knowledge indispensable to the future advancement of scientific medicine. Medical science is not limited to the diseases of man ; it embraces diseases of all organisms, and there is no branch of it which has not its important relations to every other branch. It is comparative medicine which will rank as scientific medicine of the future, but human medicine will be the practical gainer thereby. It will not be accomplished, however, by such literature as we find in the volume before us, which, although it may inspire a few to look deeper into the subject, and furnishes many valuable and practical hints, will rather serve to bolster up the pretenders to veterinary medicine.   [w. r. b.]

A Manual of Obstetrics. By A. F. N. King, M.D. With fifty-nine illustrations. Second edition. Henry C. Lea's Son & Co., Philadelphia, 1884, pp. 338.

The writer of this manual does not claim originality for his book. He has produced a compendium, or abstract, of Playfair, Lusk, and Leishman, to save his pupils the trouble. That the latter appreciate what he has done for them is evident since a second edition is issued by its publishers.

It is doubtful that the hopes of its author will be realized that "those whose onerous duties allow but little leisure for consulting larger works" will find value in its pages. Such practitioners generally have learned the simples of obstetric practice, if not from their college professors words of wisdom, from that best of teachers, Experience, and will only look for instruction in the most elaborate works, or the papers on special subjects found in current literature.

A valuable addition to the obstetric manual is the chapter on "The Jurisprudence of Midwifery," which is based on the works of Dr. Alfred Swaine Taylor and Dr. Beck. It is here that the professional man, "laden with onerous duties," might find help, since his appearance in court is not an every-day affair, and undoubtedly he could make a better show on the witness-stand if he should have the little book to read on his way thither. [g. p.]

Compendium der pathologisch-anatomischen Diagnostik. Von Dr. Johannes Orth, Prof. der allgemeinen

Pathologie und pathologischen Anatomie in Göttingen. Dritte Auflage, pp. 634. August Hirschwald, Berlin, 1884.

Prof. Orth has greatly added to this edition of his work. Sections on microscopic technique, on the examination of sputa, fæces, urine, etc., are new. The first is especially valuable as containing a summary of all that is worth knowing relative to the recognition and study of bacteria and other living disease germs. As the author himself says—these additions furnish the elements of so-called clinical microscopy.

As is logical in a work intended for use chiefly at the post-mortem table, the subjects are arranged upon the basis of topographical anatomy ; cavities, apparatuses, and organs being studied *in situ*, and then detached in the course of the systematic post-mortem. Under the head of each organ, however, follows a study of its alterations in general and in certain diseases, so that to a certain extent the work is also a treatise on pathological anatomy.

The introduction, occupying twenty-five pages, consists of general rules for making an autopsy, an enumeration of the necessary instruments, and a detailed account of microscopic and micro-chemical manipulations.

The subject-matter is broadly divided into two great parts—one treating of the examination of the external parts of the body, and the other that of its interior. Under the first subdivision we find descriptions of the attitudes and physical attributes of the corpse, a minute account of the morbid alterations affecting the skin and its epithelial appendages. One section is devoted to the condition of the new-born child. This first part occupies thirty-eight pages.

The rest of the treatise is occupied by the second part of the subject, the study of the inner parts of the body, arranged in four sections : 1. The spinal cavity and contents. 2. The cranial cavity and contents, including the eye and ear (in sixty-four pages). 3. The thoracic and abdominal cavities and their contents. 4. The extremities (including lymphatic glands, blood-vessels, nerves, muscles, joints, and bones). Particularly interesting are the sections on diseases of the cerebral membranes, on the blood, on the lungs, on sputa, on the kidneys, on the stomach, on the intestines, and on the muscles.

Orth recognizes the following forms of nephritis : (*a*) suppurative, (*b*) degenerative parenchymatous—acute and chronic (latter with extensive fatty degeneration), (*c*) glomerulo-nephritis, (*d*) granulating or acute productive interstitial, with varieties : (*d¹*)

interstitial glomerulo-capsular (cystic formation and shrinkage), (d^2) multiple chronic fibrous interstitial (syphilitic), (d^3) interstitial papillary nephritis.

The author has firm faith in the existence and significance of the bacillus Kochii : he has demonstrated its presence in sputa, in various forms of tubercular formations in the lungs, in the fine tubercles of the cerebral and spinal membranes, in the intestines, articulations, etc.

In connection with different organs the various parasites are thoroughly described.

This work is truly admirable, and will be enjoyed more especially by the working pathologist, though the advanced student and practitioners will find in it an enormous mass of facts clearly stated and grouped in such a way as to make their comprehension and retention easy. We earnestly hope that an American edition will be issued ere long. [E. C. S.]

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